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THE PSYCHOLOGICAL REVIEW

SENSATION AND PERCEPTION IN AN OBJECTIVE PSYCHOLOGY[1]

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This paper will take up what might have been called, in an earlier day, a systematic treatment of the topics of *sensation* and *perception*. In particular, it will consider their places within the formal structure of an objective or behavioristic psychology.

The development of such a psychology has been a major historical occurrence of the last 35 years, and, in some respects, it has achieved a considerable level of maturity. Despite the fact that behavioristic descriptions of perception were advanced at a relatively early date (see, for example, **9**) the area of perception still remains a difficult formal problem for systematization within the framework of an objective psychology. The reason for the refractory nature of the subject probably lies in the complexity of its relations as contrasted with the deceptive familiarity of its terminology. In any case the general topic requires analysis. Specifically this paper will discuss the following five topics: types of stimulus-response relations encountered in perception, the problem of the independent statuses of sensation and perception, the problem of descriptive and explanatory terminology, some recurring considerations in perception, and perception and psychological theory.

It is hoped that the discussion of these seemingly miscellaneous topics may take on the character of an integrated account. In all of this we shall not deal with explanatory principles as they relate to specific processes. We know that the area of perception is rich in such principles, particularly as respects vision and audition. What will be done is to consider some general characteristics of the large over-all area and its relation to other fields of psychology. The attempt will be supplemented by brief references to a symbolic notation, in terms of stimulus and response, that has been discussed previously (**3**, **4**, **5**, **6**).

TYPES OF STIMULUS-RESPONSE RELATIONS IN PERCEPTION

In analyzing the sort of behavior that can be encountered in experiments on sensation and perception, let us start with a particular type of experimental situation, one that applies to the experiment dealing with introspective reports of sensations or perceptions evoked by

[1] The preparation of this manuscript was supported by a contract between the Office of Naval Research and Columbia University. Reproduction in whole or in part is permitted for any purpose of the United States Government. The account encompasses, with minor modifications, the content of the writer's presidential address to the Eastern Psychological Association at Atlantic City on March 23, 1956.

particular objects or events involving the objects. Concretely, the question arises: What do I have to do in this situation to get a subject to give me a phenomenal or introspective report in the presence of certain objects that I supply?

Proper procedure in such a situation demands, of course, that I instruct the subject to give me a phenomenal or introspective report and that I supply the appropriate objects in their appropriate relations. The consequent behavior on the part of the subject will be the emission of a sequence of words—words that are, we presume, functions of the subject's past history of reinforcements (by parents and other persons) in a sequence of discriminative trainings as discussed, for example, by Skinner (**15**). These verbal responses are often said to be "descriptive" of stimuli. In situations such as the one described, the experimenter hears such words given by the subject as *moving, color, short, white, object, large, red, duration, size, movement, intensity, texture,* and a whole gamut of expressions that "apply" to the character of the stimuli. Since these words were conditioned in response to aspects of stimuli that are now present, they are "appropriate" responses, both to the subject and the similarly conditioned experimenter. In all of this, the place of the instruction stimuli in restricting the possible number of responses remains to be understood. To say that they provide the necessary "sets" is not very informative.

It would be of considerable interest to compare a subject's behavior under instructions to introspect or give a phenomenal description with behavior under instructions to describe objects. Certainly the subject may manifest different behaviors as exemplified in "the subjective attitude" and the "objective attitude." To what extent differences are apparent in his behavior is a question on which not very many relevant data exist at present. While the problem should not deter us, it may have later implications for a behavioristic account of perception.

In experiments of the sort under consideration—that is, experiments on phenomenal appearances or introspective reports—it is an empirical fact that the instructions used do not greatly restrict responses. The latter may exhibit a great many degrees of freedom—that is, many different words may be spoken. In addition, neither stimuli nor responses are usually measured in the observations on phenomenal appearances or introspection.

The problem of analyzing and classifying the responses in an experiment of this type is, of course, one of the major problems of psychology. In particular, the problem of analysis and classification of responses does not involve accepting them as understandable conversation, but is one of formulating their uniformities and rules in a system where they are taken *as* behavior. This sort of analysis provides the anomaly that conversation is not viewed as such, but is considered in terms of other words or symbols—that is, scientific description that formulates the rules of the subject's conversation. We deal here, of course, with the problem, discussed by Bergmann and Spence (**1**), of the subject's language and the experimenter's metalanguage.

Let us now turn to another type of experiment, one that, as contrasted with the experiment on introspection, shows measurement of both stimuli and responses as well as considerable restriction of both. Of course, this type of experiment is scientifically at a somewhat higher level of development than the introspective experiment, due to the fact that in the past its permissible stimuli and responses have been isolated and measured in one way or an-

other. In this situation the type of relevant procedural question is: What do I have to do to get a subject to say, with a certain degree of probability, "I see a light," or its converse, "I don't see a light"? The subject, in the course of any single stimulus presentation, can only give one of the two possible responses (or under appropriate conditions, their "equivalents," *Yes* or *No*).

This type of experiment is the classical psychophysical experiment. It turns out, in this situation, that light intensity must be manipulated, and we can plot some measure of response—for example, its probability of occurrence—against some appropriate function of intensity. This procedure has been followed in Fig. 1.

This figure gives the psychophysical data of three subjects for an experiment on quantum requirements at threshold by Hecht, Shlaer, and Pirenne (7). Such a function is a stimulus-response relation that shows how a measure of response varies with a controlling stimulus variable. From such a relation it is possible to obtain a new datum, a value of the stimulus variable that corresponds to a given probability of response occurrence—for example, the 50% value. This new and extremely important datum is familiarly known to us as a threshold.

The two categories of the experiment that have been discussed, the phenomenal report and the psychophysical, lie

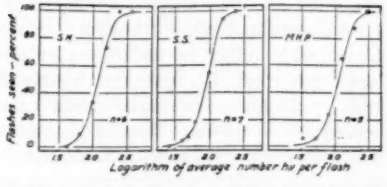

FIG. 1. Frequency of seeing functions obtained on three subjects by Hecht, Shlaer, and Pirenne (7) in their experiment on quantum requirements for visual threshold.

at the extremes of an array of psychological experimentation, the array being ordered on the basis of degree of stimulus and response restriction. The introspective experiment involves relatively unrestricted stimuli and responses, while the psychophysical experiment usually involves a high degree of restriction in both.

Other types of experiments take up positions between the extremes of such a continuum. One may have in mind particularly the type of experiment involving absolute judgments, estimates, or naming behavior. This type of experiment deals with the relation between environmental stimulus variables, often measurable in some theoretical numbering system taken, for example, from physics, and large families of variable responses. This is the sort of experiment that involves such procedural questions as: What do I have to do to make a subject say "I see five dots," or "This is red," or "This has a value of three on a scale of one to five"? The response given in this experiment is one that has developed during the subject's life history in the presence of objects. "Naming a color" and "giving a numerical estimate of brightness" are examples of such responses.

Figure 2 gives a set of hypothetical curves for color naming in the spectrum. Each curve gives, as a function of wave length, the frequency of occurrence of the response that applies to the curve.[2]

As contrasted with the method of constant stimuli with two categories, where only two responses can be elicited

[2] The curves of Fig. 2 were not drawn on the basis of data. They were constructed on the basis of "reasonable guesses" relating to other kinds of color discrimination. Since this paper was written, Aleeza C. Beare, working at Columbia, has made experimental determinations of color naming functions at two levels of intensity. The functions are, in fact, quite similar to the hypothetical ones shown in Fig. 2.

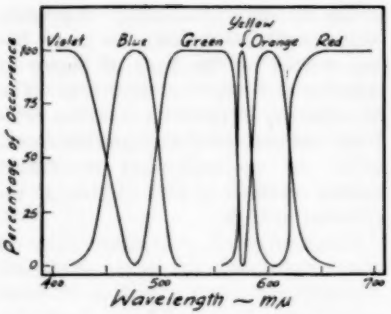

FIG. 2. Hypothetical curves for frequency of occurrence of color names in various parts of the spectrum.

no matter what the value of the discriminative stimulus, the method of absolute judgment allows for the production of a great number of responses. For example, in the naming of a color, a considerable change in wave length can result in many changes in response. Many problems beset our understanding of this sort of behavior function. In particular, we cannot specify, as yet, quantitative continua that represent relevant *amounts* of differentiated responses.

A number of undesirable ways of talking about the results of the experiment on absolute judgments hinge on this problem. Here one may mention two examples. First, there is the device, harmless enough if we keep in mind what is being done, that has sometimes been used to imply response continuity. This is the device of assuming a response continuum as the correlate of physical dimensions of the stimulus, as when we say that "orange" (rather than the wave lengths that evoke the response "orange") is next to "red" in the spectrum (rather than the wave lengths that evoke the response "red"). Secondly, there is the procedure of treating numerical estimates as if they were numerical data. For example, the word *ten* emitted by a subject in an experiment on estimated brightness may be treated, erroneously, I think, as if it were the quantified outcome of a measuring operation; the symbol for response (accepted by the experimenter as a number) is added to the list of other numbers of the same sort, and the experimenter says that an "average" response is finally computed. No independent operations of measurement have been applied under these circumstances to the subject's responses.

In any case, it should be pointed out that we can theoretically derive critical values—threshold values, if you will—from certain types of experiments on absolute judgments. For example, we can find, on the average, how many dots are required for the response *five*. We can also find out how many are required for the responses *four, three, two, one,* etc. Similarly, in the case of estimates of brightness, one can find the value of luminance that is on the average correlated with the response *ten,* for example. We do not find what the average response is for the same luminance.

The concept of threshold, i.e., a critical value of stimulus corresponding to a given response criterion, to which previous reference has been made, need not be elaborated here; it is an old and pervasive principle of psychology. Nevertheless, it will not be out of place to point out that, by virtue of this quantity, it is possible to treat as a single datum the outcome embodied in a total psychophysical function, and so derive what might be called perceptual functions—that is, functions obtained from a sequence of experiments concerned with finding thresholds under many different conditions. Such experiments pose such procedural questions as the following: What do I have to do to get a subject to say "This light is brighter than that one" under condi-

tions where I systematically vary the standard light?

Figure 3 shows such a sequence of psychophysical curves obtained by Mueller (14) in his determination of the just discriminable increment in intensity at various intensity levels. Each curve gives a threshold ΔI, the critical value of ΔI taken as corresponding to the 50% response occurrence.

In a further extension of the data, we determine how the second-order datum, the threshold ΔI, varies with changes in the standard light. The function obtained in these circumstances shows how determined values of ΔI (each one based on a psychophysical function) vary with the intensity of the standard light.

Figure 4 represents such a function. It is the curve for Mueller's threshold values of ΔI as a function of the background intensity to which ΔI is added. Such a function, which we may term a perceptual function, shows how a critical value of stimulus, the threshold, varies as a function of a controlling variable.

It is of interest to examine the present analysis in terms of a symbolic formulation that has been used (Graham,

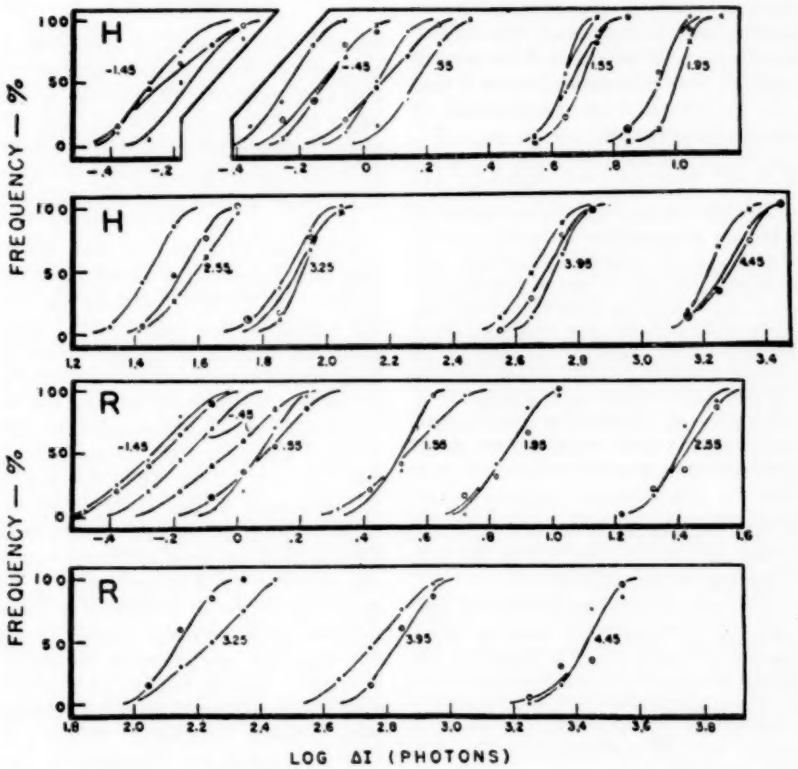

FIG. 3. Sequence of psychophysical ("frequency of seeing") curves obtained by Mueller (14) for intensity discrimination thresholds at various levels of intensity. Data for two subjects, H and R.

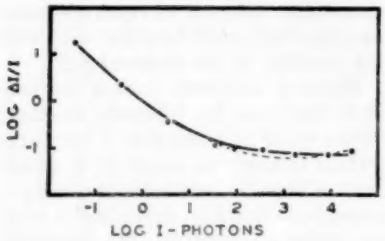

FIG. 4. Intensity discrimination ratios ($\Delta I/I$) at various levels of intensity based on the critical ΔI's obtained by Mueller (**14**) at the 50% frequency of occurrence values in the curves of Fig. 3.

3, **4**, **5**, **6**) to specify some classifications of relations among stimulus, response, and conditions of the subject. Specifically, the variables of the present analysis must be examined to see if they can be described as special cases of the general relation

$$R = f\,(a, b, c \ldots n \ldots t \ldots x, y, z) \qquad [1]$$

This relation expresses the assumption of a behavioristic program: response is a function of certain specifiable variables. In particular, the first letters of the alphabet (a, b, $c \ldots$) refer to properly specified aspects of stimuli; the last letters ($\ldots x$, y, z), to properly specified condition of the subject (physiological and inferred, including the effects of instruction stimuli); R, to response; n, to number of presentations; and t, to time. The terms are not always independent of each other (Graham, **6**, p. 62).

It is important to realize that x, y, and z may be, among other things, the consequences of past stimulation.

The stimulus-response data of a psychophysical experiment can be represented, at least for the cases of the methods of constant stimuli and limits, by the equation:

$$R = F(a, x_1) \qquad [2]$$

This equation is a special case of Equation 1: response is a function of an aspect a of the stimulus, with x_1, the effect of instructions, explicitly specified as constant. The same sort of analysis can be applied to the data of absolute judgments. Here it must be admitted, parenthetically, that to a considerable extent the discussion here presented depends for its validity on the choice of the methods of constant stimuli and limits, both with extensive series of observations. When the method of adjustment is used, response measurement becomes difficult.

A combination of psychophysical experiments provides the data for a perceptual function as in

$$a_c = \Phi(b, x_1, R_1) \qquad [3]$$

where a critical aspect of stimulus a_c is represented as a function of another aspect of stimulus b. In this case x_1, the effect of instruction, and R_1, the criterion response, are maintained constant. In other experiments, a could be determined as a function of such variables as n, t, or y.

Equations 1, 2, and 3, and similar ones give a convenient classificatory account of the types of relations found in the psychophysical experiment. What about the relations of the experiment on phenomenal appearances? Is there a distinction between the data of the introspective and psychophysical experiments? Certainly there seems to be in two respects. First, both the stimuli and responses of the introspective experiment are relatively unrestricted. They depend for their occurrences upon the behavior of a subject who is responding and being stimulated in many different ways. Secondly, neither stimuli nor responses are measured in the introspective experiment, and so a quantitative description of stimulus-response dependence is impossible. While it is true that stimuli and responses are presumably related from a rigorous mathematical point of view, the statement has little utility in the absence of knowl-

edge of the specific relation. Nevertheless we can use the reports of such an experiment at a conversational level to provide us with hypotheses to be tested in the more formalized psychophysical experiment. We may also examine the reports themselves with a view to analyzing them in the context of a descriptive or explanatory system of language.

It is to be hoped that we shall not long remain ignorant of what the subject is doing in the introspective experiment, and, in fact, in any circumstance in which he talks.

THE SEPARATENESS OF SENSATION AND PERCEPTION

Up to the present time nothing has been said about differences that may exist between sensation and perception. For all practical purposes the two topics have been considered to be one. The question now arises: Can they, in fact, be differentiated on the basis of appropriately selected criteria?

Sensation and perception were accepted as different in the general framework of a structural psychology. Sensations were taken to be the irreducible elements of consciousness; and perceptions, the carriers of meaning, were judged to be aggregates of sensations and images (Titchener, 16). Exponents of phenomenological description took another tack. They regarded their particular interest as concerned with the phenomenal world or the phenomenal field (cf., e.g., Gibson, 2; Katz, 10; MacLeod, 13), depending on the different attitudes taken in observation. Of course, Gestalt psychologists have, from their beginnings, disclaimed the concept of sensation *in toto;* they deal with stimulations and their consequent patterning (cf. Koffka, 11; Köhler, 12).

Despite the great differences exhibited by these various psychologies in the details of their treatments of conscious experience, they do demonstrate, at least superficially, an emphasis on the importance of the patterning of units in perception—units that are variously conceived to be sensations, stimulations, or cues.

In addition to their usage within the context of psychologies of experience, the terms *sensation* and *perception* are sometimes used in a different, possibly unwarranted manner. In consequence of their preoccupations with the nature of stimulation, psychologists often now use the term *sensation* to include whatever physiological theory and data of sensory processes may seem to be applicable in a given circumstance. Sensation has come to be thought of as essentially physiological, whereas perception, which is relatively poor in physiological theory, is considered to be psychological.

In view of the long historical background of the terms *sensation* and *perception,* one may legitimately ask: Are either or both terms needed in an account of behavior? It must be admitted that one could use many terms as convenient classificatory devices in discussing behavior. For example, the term *discrimination* might be used to cover the class of functions that are of interest here. It is probable, however, that the latter term would usually be interpreted in a more restrictive sense than would a term that covers the array of functions here discussed, characterized as they are by the fact that response is treated as a function of stimulus, with conditions of the subject due to past reinforcements at constant or limiting values. (Of course this statement does not mean that we are not interested in past history parameters! In the latter circumstances, response is treated as a function of conditions of the subject with stimulus constant. But of course the latter descriptions have their own designation: learning or conditioning functions.)

In any case, at least for the present, the term *perception* will be retained to refer to classes of stimulus-response functions that represent constant values of reinforcement parameters. Does a basis exist for retaining the term *sensation* independently of *perception?*

Let us examine the problem. The behavior observed in all perception experiments (introspective, phenomenological, or psychophysical) involves stimulus discriminations and response differentiations. The correlated terms in the observed stimulus-response functions are the experimental data. In experiments that provide such data, an instruction stimulus is given, ideally only once; thereafter, a stimulus aspect is changed and certain responses are observed. Variations in a controlling parameter of the stimulating circumstances give rise to correlated response occurrences or changes. This context seems to cover all of the functions classifiable under the heading of perception, with no surplus functions requiring a new heading. Thus, on the side of description of experimental operations, it seems that no additional term is needed. It is true, of course, that the needs of theory cannot be finally prejudged. It might be that some theoretical analysis could provide appropriate criteria for the use of two or, in fact, more terms to specify processes of different orders of complexity. So far the use of two terms does not seem to be based on mutually exclusive criteria. We seem to do well enough with one.

THE PROBLEM OF TERMINOLOGY

The analysis of experiments in perception and their related concepts pose an important practical and theoretical question: Shall we dispense with many of the terms of an earlier vocabulary and try to develop new terms for a behavior science, or shall we define new meanings for the old terms? It may be too early to tell whether an account of behavior can more advantageously use old terms, appropriately respecified, or new terms, divested of certain historical contexts. In any case, it will probably do no harm to see what considerations might arise from the use of one set—specifically, the old terms. Such an examination does not now imply an ultimate preference for one system as opposed to the other. It may, however, be supposed that a sequence of such examinations would at a later time provide us with a basis of choice.

Let me take some examples from the field of color. In this area it might conceivably be possible to get along without such words as *color, brightness, saturation,* or *hue,* provided that we could specify the end terms of a given discrimination. Experience, however, shows that, even if we could always (as we cannot) express uniquely the energy relations that correlate with a given response (and vice versa), the use of such terms would be cumbersome. For this reason we shall here consider the roles of such words as *hue, saturation, brightness, color* and *chromaticness.* We shall not argue the question of whether these names are labels, intervening variables, or hypothetical constructs. The applicable terms will probably be chosen on the basis of the theorist's attitude.

Consider, first, the use of the term *hue.* This term is to be understood as either a label for or as an inferred effect (behavioral, physiological, or mathematico-representational) in the following stimulus-response sequence: (*a*) instructions to a subject who has had a past history with the vocabulary represented in the instructions, (*b*) the presentation of radiant energy to the subject, and (*c*) the subject's responses. It turns out, as a matter of empirical fact, that wave length is the most important variable in (*b*) for hue discrimination.

The dependence of hue discrimination on wave length may have been established early in such observations as the ordering of stimuli by a person when he is instructed to arrange colors on the basis of hue. The subject is said to discriminate differences in hue when he gives one response (for example, a hue name) to a radiant flux of one narrow wave-length band and another response to flux of a different wave length. Another type of correlated observation involves the subject's giving (*a*) one response ("No, there is no difference in hue") to a small difference between wave lengths of a pair of stimuli, and (*b*) another response ("Yes, there is a difference in hue") to a larger difference. The threshold wave-length difference is evaluated at the appropriate frequency of occurrence of the two responses.

It will be observed that the word *hue* comes into play at least twice in the sequence *instructions, stimulus, response:* It occurs as part of the instructions (as in the statement "arrange these colors on the basis of hue"); and it is the term for the inferred effect relating stimulus and response, or the label for the relation.

The word *hue* used in the instructions (and reacted to by the subject) is analytically a different word from the word applied to the discrimination. The difference in the two words is comparable to the difference discussed by Bergmann and Spence (1) when they contrast the "social" language of the subject and the experimenter with the metalanguage of the experimenter as a scientist. Possibly, the difference could be specified by the use of subscripts, as in hue_i and hue_d, where the former word represents the term in the instructions, and the latter the term applied to the discrimination. The former term controls a subject's activity; it "tells him what to do"; it may imply little or no theoretical context. The latter term involves whatever meanings may be attached to it by its ramifying empirical and theoretical connections. Thus, for example, it may imply an elaborate context of physiological mechanisms.

Considerations comparable to those holding for the word *hue* exist with respect to the word *saturation*. The term represents the inferred effect or relational label holding between such a stimulus variable as colorimetric purity and differential responses that exist when a subject is instructed to order colors on the basis of, for example, "paleness of color."

Differences in brightness may be discriminated by processes analogous to those holding for hue discrimination. The subject, under instructions to arrange stimuli according to differences in *brightness* or to indicate *brightness differences,* gives different responses (for example, the words *dim* and *bright*) to different luminances, or he may differentially signal a difference between luminances. Such discriminations are said to be *brightness discriminations.* The word *brightness* appears in the double sense characteristic of the word *hue.*

The word *color* represents a generic type of discrimination based on a combination of wave length, colorimetric purity, and luminance. The subject, properly instructed, can respond by naming different colors or with such differential responses as "Same color" or "Different."

The term *chromaticness* is of considerable interest, based as it is on a very definite type of representational theory: the chromaticity diagram. Chromaticness theoretically represents a generic type of discrimination based on a combination of wave length and a quantity called excitation purity. It thus may be taken to be a combination of hue and saturation, with brightness removed

from consideration. As a concept it is based on a highly abstract mathematical representation, and its correlated responses have not as yet been carefully considered.

The word *perception* itself requires some discussion. The word does not in itself refer to an inferred effect having the significance of an intervening variable (as has been the case with the words previously discussed). It is a class name for all such variables. Presumably, it is to be understood as the class of all relations conventionally subsumed under the word *perception*. (We shall not here go into the problem of formal bases for this subsumption.) In any case, we should not expect to hypothesize about a generic *process* of perception as we might about a specific component process.

This discussion of representative terms in the field of color, some being taken directly from an older psychology, will probably show that the latter terms can be used in the vocabulary of a behavior science. Whether they will serve there more suitably than new terms is an open question. Would it be better, for example, to speak of wave-length discrimination rather than hue? On this problem at least one thing may be said: Comparable aspects of wave-length discrimination show different dependencies on wave length as functions of other variables. Put in another way, hue varies with other factors than wave length. The difficulty posed by this fact can be surmounted, but possibly only at the cost of some terminological cumbersomeness.

SOME RECURRING CONSIDERATIONS

Let me now take up some recurring considerations in the study of perception. In particular, I feel that the old empiricistic-nativistic controversy may still have some implications for present-day studies. I am not pleading for a renewal of this ancient argument, but do desire a clarification of some of its concepts in the interest of benefiting our present understanding.

Any situation that involves a discrimination, an estimate, a denotation, or a naming response employs a subject with a system of responses. Many, or most, of the responses are the end-products of the subject's history of acting in a complex and changing environment. Now it is never shown in researches on the intact organism that the subject's past history is irrelevant. What may be shown is the fact that, in certain situations, past history variables do not establish the *form* of a given function. In a word, such variables exist at constant values. Under other circumstances, they do influence the form of a function; they are contributing variables. The identification of both classes of function is a matter for experiment.

Past history variables are important in some types of perceptual behavior, and they are now receiving increased attention. Studies of motivational factors in perception are being conducted at an accelerated pace. Despite some criticisms that can be made of certain recent experiments, nevertheless, in general they support a permissible modern interpretation of Helmholtz's position (8) on unconscious inference: discriminative behavior is influenced by past conditions of the subject. The study of the latter effects is in one of the two major directions of perceptual investigation. In pursuing this line, answers may be sought to such questions as the following: What do genetic studies tell us about the development of certain discriminations? What sorts of training procedure can be shown to influence discriminations and how do they do it? How will different instructions change the responses of the subject? What are

appropriate dimensions for the quantification of instructions?

The other major rank of perceptual study is, of course, an old one. It is concerned with the problem of how systematic variations in stimulus conditions influence discriminations. This area has had its problems and controversies, but we need not revive them now. It is sufficient to say that the necessity for advancing our knowledge of the energy bases of discrimination to more refined levels remains with us. It is true that some quantitative experiments have been done in the past, but it is also true that experimental parameters have not always been varied over a large range and in a systematic manner. Put briefly, many perceptual experiments of the past have often been demonstrational rather than analytic, particularly in cases where variables other than intensity and wave length have been the main centers of interest. One thing the study of perception needs is adequate data to provide a firm base for theory. In the future it may probably turn out that many interesting demonstrations may be seen for what they are—special cases of much more general quantitative principles.

PERCEPTION AND PSYCHOLOGICAL THEORY

It seems interesting and important that we can go a considerable way in classifying some of the discriminative functions in perceptual behavior and that we may designate two directions which appropriate studies may take: investigating (*a*) conditions of the subject, and (*b*) stimulus conditions.

It is probably true that the studies of conditions of the subject (in the first direction) must be brought into relation to learning and conditioning. To put it briefly, such studies must make meaningful contributions to our understanding of behavior modification, the area where past history variables are of primary concern.

Studies of the stimulus control of behavior (in the second direction), when properly made, usually do not show the influence of past history as a contributing variable. The experimentalist and theorist in this area has the responsibility of relating his findings to an already existing precise body of formalized information. In a sense, then, the experimentalist in this area must do more than classify the behavior functions. He should be sensitive to the need for relating his findings to appropriate theory, sometimes in the vocabulary of psychology, sometimes in the vocabulary of physiology, and often in the terminology of mathematics. By his contributions the psychologist interested in the stimulus control of behavior can provide sounder bases for the interpretation of the often admirably conceived but disconnected hypotheses that now exist, and, in a larger context, advance the synthesis of these hypotheses into a more inclusive theory.

Finally, it is clear that an ultimate synthesis of information obtained in future investigations—i.e., from studies of (*a*) conditions of the subject and (*b*) the stimulus control of behavior—will be required for purposes of a more embracing theory. Data from the first direction will tell us how discriminative behavior develops and what are the conditions for its development; and data from the second direction will tell us how such discriminations are controlled and limited. Such a synthesis will make the study of perception coextensive with the general study of behavior.

SUMMARY

This paper discusses five topics that are of systematic importance in the treatment of sensation and perception from the point of view of an objective psychology.

(1) Types of stimulus-response relations are considered as they relate to the following topics: (*a*) introspective or phenomenological descriptions, (*b*) psychophysical discriminations, (*c*) absolute judgments, and (*d*) thresholds and perceptual functions. Only (*b*) and its derivative (*d*) usually involve measured aspects of both stimulus and response. In the case of (*c*), the response is usually not quantitative. In the case of experiments involving introspection or phenomenological description, neither stimulus nor response is quantitative.

(2) The terms *sensation* and *perception* probably do not refer to two different operationally specifiable concepts.

(3) Problems of terminology are considered. Such terms as *hue, brightness,* and *saturation* are examined and analyzed. It would probably be possible in a behavior science to dispense with such terms, but they may have the advantage of reducing cumbersomeness. Their meanings within a context of stimulus-response relations are considered.

(4) Some recurring problems of perception are described, with particular reference to the kinds of variables involved. It is stated that the two major directions of perceptual investigation involve studies of the effects on discriminations of (*a*) stimulus conditions and (*b*) past history parameters and conditions of the subject.

(5) Analysis of the classes of stimulus-response functions concerned in perception indicates that the study of the latter topic becomes coextensive with the general study of behavior.

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TOWARD A QUANTITATIVE DESCRIPTION OF LEARNING SET DATA

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When the same monkey, ape, or child is given hundreds of object-discrimination problems to solve, he gets better and better at mastering them. Controls in the basic experiments eliminate two attractive explanations of this improvement. The cumulative transfer of training is *not* based on cues common to and peculiar to the correct objects, for problems are made up by random pairings from a pool of mutually discriminable objects. The cumulative improvement is not likely to be based solely on habits of conforming to the procedure of the experiment. Monkeys typically receive months of preparatory training in moving objects, in picking up rewards, and in general "good behavior" before the learning-set (LS) experiment itself begins.

It would appear, to the naive observer, that monkeys in an LS experiment "get the idea" of solving discrimination problems. An adult human subject, trained in this fashion, would soon realize that of the two objects presented in any problem, one is rewarded and the other not. He would learn that when new objects appear, the correct response is unpredictable until after one information trial, but that, in subsequent trials with the same objects, reward is consistently given and predictable. The data suggest that monkeys also respond in this fashion, though they acquire this general understanding of the situation only very slowly.

If we had available only the observation that monkeys improve in successive discrimination problems, it would be tempting to try an explanation in terms of the simple processes characteristic of naive rats. The compounding of further detailed information about the performance of sophisticated monkeys and apes makes such a reduction seem almost impossibly difficult. But an attempt to formulate the intuitively sensible hypothesis that monkeys gain an abstract understanding of the situation puts a great strain on current theoretical resources. Cognitive discussions of learning and performance are validly criticized on the ground that they do not yield precise predictions regarding behavior and thus are not conveniently verifiable. In the present paper, an attempt is made to formulate a descriptive theory of LS formation and operation which centers on an assumption of abstract cognition, and yet yields exact quantitative predictions which can be, and in some examples below will be, compared with the details of monkey performance.

The theoretical aim is to summarize a variety of data, using a few assumptions. To accomplish this kind of parsimony the writer has found it expedient to violate another informal canon of learning theory: that processes invoked should be simple and familiar. It is assumed that monkeys use an abstract understanding of the

[1] The author is indebted to H. F. Harlow, J. M. Warren, and A. J. Riopelle for many valuable criticisms and insights and for making a variety of unpublished findings available.

[2] Part of the research on which this paper is based was done while the author was in residence at the Center for Advanced Study in the Behavioral Sciences.

pattern of an LS experiment, transcending the "stimulus-response" rubric familiar in most theories of learning.

Since the present formulation grew out of attempts to extend an earlier theory of discrimination and choice learning (**10, 11, 12**) to LS experiments, it is not surprising that the LS theory contains the earlier theory as a special case. However, it would be wrong to consider this LS theory a deduction from the simple discrimination learning theory. The logical implication is from the LS to the simpler theory, not *vice-versa*.

Empirical Scope of the Theory

The present theory is intended to describe the change in height, rate, and shape of within-problems learning curves with successive problems. While the general characteristics of the data are represented in the theory, complications arising from our ignorance about how the stimulus objects appear to monkeys, and indeed about the actual stimulus properties of the objects, make it difficult to determine whether or not the theory is correct. In addition, as we shall see, there is evidence that the theory is incomplete, leaving out one of the factors which result in improved performance.

The formulation may also be related to reversal learning-set, and the solution of problems like oddities, matching from sample, delayed reaction, etc. These other problems can be thought of as involving (for the monkey) abstractions like, but different from, those involved in ordinary discrimination LS. The theory has not been elaborated enough to permit detailed analysis of these problems, and the present paper will merely sketch the possibilities.

Most important, for quantitative evaluation of the theory, is its use in analyzing the performance of sophisticated monkeys on various perceptual problems. After extended training the monkey reaches a relatively stable state, in which his performance on a variety of tasks can be studied intensively. It happens that the equations describing performance in this stable state are quite simple. Since the theory purports to relate learning to perceptual characteristics of the situation, it is possible to frame some interesting, if oversimplified, perceptual hypotheses, and then use the theory to evaluate them. Though these tests of the theory are separate from a large part of the general apparatus of the theory, and involve extraneous hypotheses about the stimuli, they are helpful in evaluating the theory because the interpretation of the data hinges on the crucial assumption of the whole structure—the relation between the perceptual situation and the rate of learning.

Theory of Learning Sets

The fundamental assumptions of the theory are: (*a*) Solving a discrimination problem involves two processes: conditioning valid cues to the correct response, and adapting or suppressing invalid cues. (*b*) These processes can be quantitatively described as the cumulative effects of a random process. (*c*) The rates of these processes (that is, the proportion completed on each trial) depend on the mean validity of cues present. (*d*) The probability of a correct response is the proportion of functional (unadapted) cues conditioned to the correct response. These assumptions have been stated in earlier papers (**10, 11, 12**).

In learning-set experiments involving higher primates, there exist certain special cues, which we shall term "type-*a*" cues, which are valid (con-

sistently reinforced) and common to all problems of the experiment, remaining constant when the particular stimulus objects are changed. These cues are present on every trial except the first (information) trial of each problem, and they depend on previous reinforcements. In effect, a monkey responding to a type-*a* cue is responding to "the object which has been reinforced." He is responding to the property of having been reinforced, as distinguished from other properties such as size, color, spatial shape, arrangement of parts, etc.[3] These latter concrete properties give rise to type-*b* cues, which are valid (consistently reinforced) during a given problem, but which are not valid in the long run. There are other cues in the situation, arising from the apparatus, certain characteristics of the procedure, and, most important, from within the animal itself. These cues, termed type-*c*, are not reinforced consistently at any time, and thus are invalid.

Indications are that in the monkey, type-*a* cues make up a very small proportion of the cues the animal can use. Type-*b* make up a much larger proportion, about one-third of the cues, and type-*c* cues make up the rest, nearly two-thirds, in ordinary object-discrimination problems of the Wisconsin type (**4**). During any one problem, type-*a* and type-*b* cues become conditioned to the correct response and type-*c* cues become adapted, so that eventually the animal can attain perfect performance on the problem. But slowly, as more and more problems accumulate, type-*c* cues become adapted permanently, so that they have no effect on performance. Learning within a problem is then faster, since it need not wait upon the adaptation of the type-*c* cues. Then, even more gradually, type-*b* cues become adapted,[4] since they are not as valid as the type-*a* cues. When this process is nearly completed, the animal is responding in terms of the abstract type-*a* cues only. Since, by theory, these cues do not change from problem to problem, the animal does not learn new problems, but merely applies his previous knowledge again and again. As soon as one of the objects gains the property of "having been reinforced," through the one information trial, the animal responds perfectly and continues his accurate response for the rest of that problem.

Thus, this theory describes the fact that monkeys and higher primates (including children) become capable of one-trial solution of object-discrimination problems (**4, 6**). The accomplishment of this skill is described as a gradual process, again in agreement with the facts (**4, 6**). But more precise evaluation of the ideas depends on a more specific statement of the theory, in quantitative terms. We now turn to this problem.

The quantitative assumptions of this kind of discrimination learning theory are stated elsewhere (**10, 11, 12**). The present theory is identical with the most recent statement of these assumptions (**11**), except for a refinement of the concept of adaptation. Only a very brief resumé, as needed for this paper, is given here.

Any cue, k, is conditioned to one

[3] Verbal statements of the principle, by young children, are described by Hayes, Thompson, and Hayes (**6**).

[4] Riopelle has stated, and supported by experiment, the idea that highly sophisticated monkeys are not bound to concrete characteristics of the objects. In particular, he showed that such experienced monkeys rapidly forget previous experiences with particular objects (**13**). Limitations on the asymptotic performance of cats have been attributed by Warren to inability to go beyond the particular stimuli (personal communication, 1957).

or the other of the alternative responses. The probability that k will become conditioned to the reinforced response is θ, the mean validity of cues present. If K is the total set of cues present, V_k is the validity and θ_k the weight of cue k, then

$$\theta = \sum V_k \theta_k (k \text{ in } K) \qquad [1]$$

If $F_{(k,n)}$ is the probability that cue k is conditioned to the correct response on trial n, and the correct response is rewarded, then

$$F_{(k,n+1)} = F_{(k,n)}(1 - \theta) + \theta \qquad [2]$$

whereas if the other (incorrect) response is reinforced,

$$F_{(k,n+1)} = F_{(k,n)}(1 - \theta) \qquad [3]$$

is the correct response is reinforced for a block of j trials, $n + 1$, $n + 2$. . . . , $n + j$,

$$F_{(k,n+j)} = 1 - [1 - F_{(k,n)}](1 - \theta)^j \qquad [4]$$

This function, and its relation to a cumulative random process, are explained by Estes and Straughan (**2**).

In applications, we shall assume that the type-a cues are consistently reinforced on every trial except the first of each problem. If, at a certain trial of the experiment, there have been N such trials,

$$F_{(a,N)} = 1 - [1 - F_{(a,1)}](1 - \theta)^{N-1} \qquad (a = \text{a type-}a \text{ cue}) \qquad [5]$$

Since these experiments involve thousands of trials, and θ is sizeable (at least over .10 in most applications), $F_{(a,N)}$ soon approaches 1.

We shall assume that type-b cues are reshuffled at each new problem, so that at the first trial of each problem some unknown proportion of them are conditioned to the correct response and the rest to the wrong response. Learning then progresses regularly during the problem. It is convenient to describe a trial as the nth trial of the current problem. Then

$$F_{(b,n)} = 1 - [1 - F_{(b,1)}](1 - \theta)^{n-1} \qquad (b = \text{a type-}b \text{ cue}) \qquad [6]$$

Because the conditioning depends both on the total noninformation trials (N) and the number of trials on the current problem (n), we shall write $F_{(k,N,n)}$ as the probability that cue k is conditioned to the correct response at trial N, n, the Nth noninformational trial of the experiment, the nth in the current problem.

Since type-c cues are reinforced at random throughout the experiment, they cannot become conditioned consistently to the correct response. It is convenient for present purposes merely to state that

$$F_{(c,N,n)} = .5 \quad (c = \text{a type-}c \text{ cue}) \qquad [7]$$

Adaptation of cues presents a somewhat more complicated problem, especially since some recent results of Hammer (**3**) have shed new light on the nature of learned perceptual selectivity. Previously, it has been stated that invalid cues become "adapted," suppressed, or nonfunctional (**10, 11, 12**). But Hammer showed that if certain invalid cues are adapted in the presence of some valid ones, and then the valid cues are changed, the invalid cues recover their function and are "de-adapted." Or, at least, there is no positive transfer of training. This suggests that perceptual selectivity is a more delicate process than mere adaptation of invalid cues. It appears that invalid cues are adapted *with respect to* valid cues present, and their status as nonfunctional depends on the presence of the valid cues. To say that invalid cue i is adapted with respect to valid cue v is to say that, when both are present, cue i has no effect on per-

formance. If cue i is adapted with respect to several valid cues, then it is nonfunctional whenever it appears along with any of the valid cues.

The probability that cue i will be adapted with respect to cue v depends on the relative weight of cue v in the whole set, and also on the difference between i and v in validity. Specifically, we may write this probability as $\phi_{(i/v)}$. Letting θ_v be the relative weight of cue v, and V_v and V_i be the validities of cues v and i, then

$$\phi_{(i/v)} = \theta_v(V_v - V_i) \qquad [8]$$

Letting $A_{(i/v,N,n)}$ be the probability that cue i is adapted with respect to cue v at trial N, n, we have

$$A_{(i/v,N+1,n+1)} = A_{(i/v,N,n)} \times [1 - \phi_{(i/v)}] + \phi_{(i/v)} \qquad [9]$$

This formulation is a refinement of a previous statement regarding adaptation of cues under conditions of probable reinforcement (**11**).

The significance of the idea that cues are adapted with respect to specific other cues appears when we consider the adaptation of type-c cues in learning-set experiments. Within each problem, if it is solved, all type-c cues are adapted. But this seems to have little immediate effect on the next problem. The reason is that type-c cues are in the main adapted with respect to type-b cues, which are changed for the next problem. When this change is made, the type-c cues are released at once from adaptation, and have to be adapted all over again. When type-c cues happen to become adapted to type-a cues the adaptation is permanent, because the type-a cues do not change. Thus, within each problem the type-c cues are fairly rapidly adapted, but over many problems they gradually become permanently adapted.

Reducing these observations to difference equations, we have

$$A_{(a,N,n)} = 0 \qquad [10]$$

Type-a cues are not adapted because there are no other, more valid cues present with respect to which they can be adapted. Type-b cues have a certain validity, V_b, which is less than 1 because such concrete cues are not consistently reinforced over the entire experiment. Type-b cues can be adapted only with respect to type-a cues, so we may write $\phi_{(b/a)} = \theta_a(V_a - V_b)$. Assuming that $V_a = 1$, since type-a cues are consistently reinforced, we have

$$A_{(b/a,N+1,n+1)} = A_{(b/a,N,n)}[1 - \theta_a \times (1 - V_b)] + \theta_a(1 - V_b) \qquad [11]$$

Solving Equation 11 involves a conceptual problem. If a certain type-b cue is present only in some problems (as must be expected from the structure of a LS experiment), then the over-all probability that a certain cue k_b is adapted on trial n becomes $r\phi_{(b/a)}$, where r is the probability that k_b will be present on a given trial. For present purposes it is not feasible to estimate r, and we shall assume that $r = 1$. We are assuming that the several problems are made up by reshuffling a single set of type-b cues. This is certainly not exactly true. However, Riopelle (**15**) trained monkeys on LS with a minimal number of stimulus objects (that is, with r near 1), and then transferred the animals to ordinary LS (with r presumably lower). The absence of any consistent decrement in transfer indicates that LS is quite general, and that r is approximately 1. In "concept-formation" experiments with humans (**7**, pp. 232–241), perfect performance is often maintained despite continuous changes in the particular

stimuli presented. This again indicates that variations in stimulus objects may be more like reshuffling than one would think.

If essentially the same type-*b* cues appear in most problems, we should apply Equation 11 on every trial where type-*a* cues appear. The solution is

$$A_{(b/a,N,n)} = 1 - [1 - \theta_a(1 - V_b)]^{N-1} \quad [12]$$

Adaptation of type-*c* cues is somewhat more complex, since they can be adapted with respect to either type-*a* or type-*b* cues. The probability that any type-*c* cue will be adapted to a type-*a* cue on trial n, given that it is not already adapted to a type-*a* cue, is θ_a, since $V_a = 1$ and $V_c = 0$. The probability that the type-*c* cue will be adapted with respect to a type-*b* cue, given that it is not already adapted to a type-*b* cue, is $V_b\theta_b$. This statement may be related to a "cue-sampling" notion by imagining that a given cue can be adapted with respect to both type-*a* and type-*b* cues; however, on a given trial, only one type of adaptation can occur. A cue cannot go from the state of being adapted with respect to neither *a* nor *b* cues into the state of being adapted with respect to both, in the space of a single trial.

With this formulation we get the difference equations

$$A_{(c/a,N+1,n+1)} = A_{(c/a,N,n)} \times (1 - \theta_a) + \theta_a$$

and [13]

$$A_{(c/b,N+1,n+1)} = A_{(c/b,N,n)} \times (1 - V_b\theta_b) + V_b\theta_b$$

Then the probability that *c* is adapted with respect to at least one of the two types of cues, $A_{(c/a+b,N,n)}$, follows the difference equation

$$A_{(c/a+b,N+1,n+1)} = A_{(c/a+b,N,n)} \times (1 - \theta_a - V_b\theta_b) + (\theta_a + V_b\theta_b)$$

so that

$$A_{(c/a+b,N+j,j)} = 1 - [1 - A_{(c/a+b,N+1,1)}] \times (1 - \theta_a - V_b\theta_b)^{j-1} \quad [14]$$

Earlier discussion of adaptation suggests that, when a new problem is introduced, type-*c* cues adapted to type-*b* cues only become released from adaptation, whereas cues adapted to type-*a* cues remain adapted. Then

$$A_{(c/b,N+1,1)} = 0$$

so that

$$A_{(c/a+b,N+1,1)} = A_{(c/a,N+1,1)}$$

which from Equation 13 can be shown to be

$$A_{(c/a,N+1,1)} = 1 - [1 - A_{(c/a,1,1)}](1 - \theta_a)^N$$

No cues are adapted at the beginning of the whole LS experiment. Thus,

$$A_{(c/a,1,1)} = 0$$
$$A_{(c/a,N+1,n)} = 1 - (1 - \theta_a)^N$$

Substituting this expression into Equation 14, we have, finally,

$$A_{(c/a+b,N+j,j)} = 1 - (1 - \theta_a)^{N-1} \times (1 - \theta_a - V_b\theta_b)^{j-1} \quad [15]$$

According to the discrimination theory (**11**), the probability of a correct response is the proportion of unadapted cues conditioned to the correct response. This probability, which we here call $p_{(N,n)}$, is given by

$$p_{(N,n)} = \frac{\sum F_{(k,N,n)}[1 - A_{(k,N,n)}]}{\sum[1 - A_{(k,N,n)}]} \quad \text{(all cues } k\text{)} \quad [16]$$

which may be expanded using equations 5, 6, 7, 10, 11, and 15, as

$$p_{(N,n)} = \frac{\theta_a[1-(1-F_{(a,1,1)})(1-\theta)^{N-1}]+\theta_b[1-(1-F_{(b,N,1)})(1-\theta)^{n-1}] \times [(1-\theta_a(1-V_b)]^{N-1}+.5\,\theta_c(1-\theta_a)^{N-1}(1-\theta_b V_b)^{n-1}}{\theta_a+\theta_b[(1-\theta_a(1-V_b)]^{N-1}+\theta_c(1-\theta_a)^{N-1}(1-\theta_b V_b)^{n-1}} \quad [17]$$

While Equation 17 is large and complicated in form, it contains only three main independent parameters, each of which may be manipulated separately. The parameters are θ_a, the proportion of type-*a* cues; θ_b, the proportion of type-*b* cues; and V_b, the validity of the type-*b* cues. The other parameters, θ_c and θ, are simply defined in terms of the three independent ones, for $\theta_c = 1 - \theta_a - \theta_b$, and $\theta = \theta_a + \theta_b V_b$. If data on many problems are to be averaged, we may let $F_{(a,1,1)}$ and $F_{(b,N,1)}$ be one-half.

Equation 17 purports to give the probability of a correct response on any trial of a learning-set experiment. Tests of the theory will use Equation 17 and certain simplified special cases of it.

APPLICATIONS OF THE THEORY TO DATA

The first question is whether Equation 17 properly describes the set of learning curves arising during the formation of learning sets. The complicated form of the equation precludes the use of standard curve-fitting methods to test the theory. The more refined stochastic-model methods are equally inaccessible, for the same reason. The only method of evaluating the theory is to evaluate the parameters θ_a, θ_b, and V_b by a method of approximations, and set Equation 17, with those approximate parameters, alongside empirical performance curves. Harlow (**4**) has published learning-set functions for a group of monkeys learning object discriminations. To a fair approximation, these data are fitted by Equation 17 with

$$\theta_a = .0075$$
$$\theta_b = .350$$
$$V_b = .70$$

While these parameters are clearly only approximate, the correspondence between Equation 17 and the data is quite good, as shown in Fig. 1.

Inspection of Fig. 1 reveals that the changes in height, slope, and shape of the within-problems curves are followed by the theoretical function.

However, a second computation was performed on pattern-discrimination data published by Harlow and Warren (**5**). Here, it was impossible to get a good fit, because the rate of within-problems learning, based mainly on θ_b, seems to increase during learning for the monkeys. The theory tacitly assumes that θ_b, the proportion of type-*b* cues, is constant throughout the experiment. It appears that the monkey's attention is initially directed to the edges of the *objects* presented, and thus away from the *patterns* drawn on those objects, since the patterns are centered on the objects. The monkeys probably acquire some sort of "observing response" (**23**), directing their visual fixations toward the centers of the objects. If this should happen, it would increase the relative weight of the pattern cues (type-*b* cues) so that θ_b would increase during the experiment.

The success of Equation 17 for object-discrimination, where no special

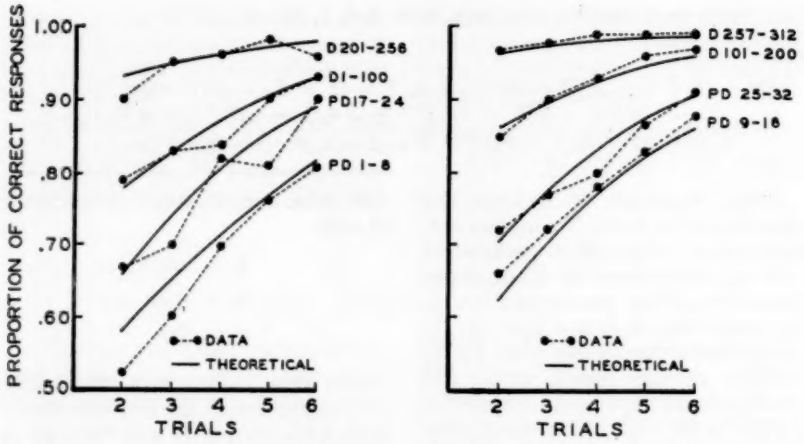

FIG. 1. Comparison of theoretical learning set function, Eq. 17, with $\theta_a = .0075$, $\theta_b = .350$, $V_b = .70$, and some data from Harlow (**4**). Alternate sequences of problems are shown in two panels to prevent crowding.

visual habits are required, and its failure for pattern discrimination, where the monkey must overcome a pre-experimental predilection, indicate that the present theory might be used to isolate the formation and effects of observing responses.[5]

In ordinary learning set, the type-*a* cues are conceived to be abstract conditional discrimination cues—"abstract" because the animal can learn to respond independently of the concrete, type-*b* cues present and "conditional" because the object chosen is contingent on previous reinforcements or nonreinforcements of those objects. In discrimination learning-set experiments, response to such abstract, conditional cues is consistently reinforced. In discrimination-*reversal* experiments, the object recently reinforced becomes the wrong object, abruptly, in the middle of the experiment. The monkey must learn a new contingency, for he must learn to reverse objects to the cue of an unexpected nonreinforcement. That reversals can be cued, by controlled stimuli, has been demonstrated in an experiment by Riopelle and Copelan (**16**). The mechanism to be suggested here for reversal learning set is essentially the one they were analyzing—another type of cue, which might be called type-a_r, which in reversal learning can come to supercede regular type-*a* cues. Since, right after reversal, the animal would have no clear cue based only on type-*a* cues (which object, after all, is the one which "has been reinforced"?—both objects have been reinforced), it is reasonable to believe that some more fitting and precise abstract cue may arise to control behavior.

The present theory relates rather directly to Meyer's equations for discrimination and reversal learning (**8, 9**). Meyer noted that within-problems learning curves from fairly

[5] A theory of learning set, which depends mainly on observing responses as the mechanism, is possible. In the present theory, observing responses are considered possible special influences on behavior, but do not play a central explanatory role.

sophisticated animals are exponential in form, with a constant rate but a gradually increasing origin. If, in Equation 17 above, we assume that the type-c cues are almost all adapted (i.e., $A_{(c/a+b,N,n)}$ is almost 1) and the type-a cues are all conditioned to the correct response ($F_{(a,N,n)} = 1$), then

$$p_{(N,n)} = \frac{\theta_a + \theta_b[1 - (1 - F_{(b,N,1)})(1 - \theta)^{n-1}][1 - \theta_a(1 - V_b)]^{N-1}}{\theta_a + \theta_b[1 - \theta_a(1 - V_b)]^{N-1}} \quad [18]$$

Since the adaptation of type-b cues is very slow relative to the conditioning of them, a given within-problems learning curve would be very closely approximated by considering $[1 - \theta_a(1 - V_b)]^{N-1}$ a constant. Elementary algebra shows that in this case

$$p_{(N,n)} = 1 - \beta(1 - \theta)^{n-1} \quad [19]$$

where β depends on the degree of adaptation of type-b cues and also on the animal's original preference for one object or the other. If almost all type-b cues are adapted, β becomes quite small. Since Equations 17, 18, and 19 all include type-a cues as present, of course, we must assume that we are talking, not about an information trial, but some later trial (Trial 2, 3, etc.) in the problem. Equation 19 shows that the within-problems learning curve, for a fairly sophisticated animal, will be nearly exponential in shape. It is also worth noting that the rate of this exponential curve depends on θ, which is a constant except for possible observing responses. Meyer's observation was that within-problems learning curves were roughly exponential, with a constant rate. His rate, estimated from his own data, was .251, which corresponds closely to the rate we selected for Harlow's data. In Harlow's data $\theta = \theta_a + \theta_b V_b$ was $.0070 + .350(.70) = .2520$. The gradual increase in the intercept, or performance on the first postinformation trial, according to the present theory depends on adaptation of type-b cues, and a corresponding decrease in β.

In an oddities problem, the monkey must select whichever object, of three or more, is the odd one. His performance will be no better than chance if he selects the object which last was reinforced, since that may not be odd. Monkeys can solve such problems, presumably on the basis of an appropriate abstract cue, but the formation of a discrimination learning set seems to be of little help, and perhaps a hindrance. This observation checks with the theoretical interpretation.

It should be clear that further analysis of data from experiments on reversal learning, oddities, and so forth, is needed. The general method of formulation suggested by this theory is to hypothesize a kind of type-a cue corresponding to the procedure employed, taking account of possible cues the monkey could use to solve the problem. These conditional, abstract cues can be evaluated as to their relative weight (θ_a), using Equation 17 or reasonable modifications thereof. Interrelations between the various abstract cues are sometimes apparent from inspection. A number of tests of the theory are possible along these lines, and may be worth pursuit.

Equation 19 is the basis of other tests of the theory. Using this equation, we can analyze the performance of monkeys which are fairly sophisticated. The important idea is that in Equation 19, the parameter θ depends mainly on the proportion of type-b cues. This is so because $\theta = \theta_a + \theta_b V_b$, and θ_a in the monkey seems to be

very small compared with θ_b. To an approximation, then, $\theta \cong \theta_b V_b$. It should be recalled that type-*b* cues are concrete, differential cues. Differences between the discriminanda in color, size, form, or other properties are represented as sets of cues of type *b*. The relative amount of valid differential cues theoretically controls θ in Equation 19.

An example of the use of Equation 19 is in an experiment on additivity of cues, performed by Warren (**21**). The same monkeys solved a variety of pattern discriminations in which the differential cues were form (F), color (C), size (S), or combinations of two characteristics (F + C, F + S, C + S), or combinations of all three dimensions (objects differing in F + C +S). At the beginning of testing the animals were fairly sophisticated.

Assuming that β would be the same for all the various problems, it was possible to estimate the parameters for the F, C, and S problems. The parameters selected were

$$\beta = .35$$
$$\theta_F V_F = .089$$
$$\theta_C V_C = .221$$
$$\theta_S V_S = .067$$

These parameters were selected by visual curve-fitting, β depending on the intercept and θV depending on the learning rate.

Assuming that in these problems, as in object discrimination, the validity of type-*b* cues is about .70, so that $V_F = V_C = V_S = .70$, the above equations can be solved for θ_F, θ_C, and θ_S, the proportions of type-*b* cues in the three types of problems. The values obtained are

$$\theta_F = .127$$
$$\theta_C = .316$$
$$\theta_S = .096$$

From these parameters, we seek to predict performance on multiple-cue problems, that is, the F + C, F + S, C + S, and F + C + S problems. We first note that if we let I be the number (or weight) of type-*c* cues, F be the weight of form cues, C be the weight of color cues, and S be the weight of size cues, we have the relationships

$$\theta_F = F/F + I$$

or

$$F = I\theta_F/(1 - \theta_F) = .146I$$
$$C = I\theta_C/(1 - \theta_C) = .462I$$
$$S = I\theta_S/(1 - \theta_S) = .106I$$

We now note that if form, color, and size cues are additive,

$$\theta_{F+C} = F+C/F+C+I = .146I + .462I/.146I+.462I+I = .379$$

Similarly,

$$\theta_{F+S} = .146I + .106I/.146I + .106I + I = .201$$
$$\theta_{C+S} = .362$$
$$\theta_{F+C+S} = .416$$

By assumption, V in all these cases is .70, so we may at once compute the predicted learning rates for these combined-cues problems. They are

$$\theta_{F+C} V_{F+C} = .265$$
$$\theta_{F+S} V_{F+S} = .141$$
$$\theta_{C+S} V_{C+S} = .253$$
$$\theta_{F+C+S} V_{F+C+S} = .292$$

In all cases, we shall continue to assume that $\beta = .35$. Now, we have computed complete predictions for the four multiple-cue problems, without reference to the data of such groups. For example, the learning curve for $F + C$ problems should be

$$p_{(N,n)} = 1 - .35(1 - .265)^{n-1}$$

for all trials, n, except the first. This prediction can be compared with the

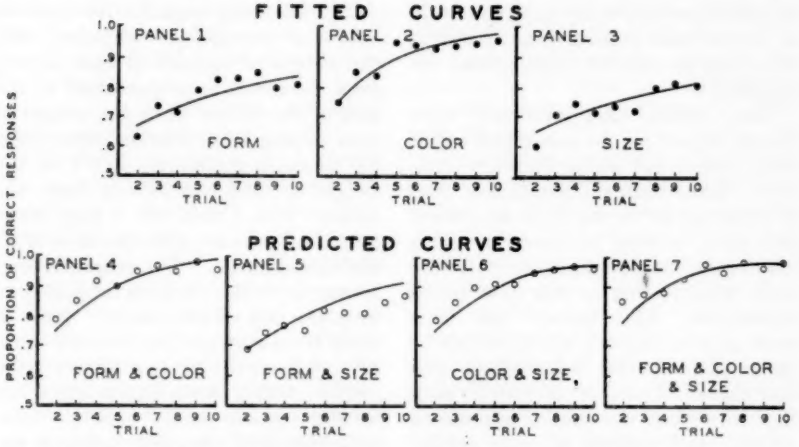

FIG. 2. Fitted within-problems learning curves for single cues, and predicted curves for combined cues, in Warren's experiment on cue-additivity. Panels 1, 2, and 3 show curves fitted by approximation. Panels 4, 5, 6, and 7 show a priori curves. All curves are from Equation 19. For parameters and rationale, see text. Data from Warren (21).

performance of the animals on the F + C problems.

The fitted curves for the three separate cue problems, and the predicted curves for the combined-cues problems, are plotted in Fig. 2. Inspection shows that the predictions are quite successful, almost as good as the fitted curves with which we started.

The fact that the empirical curves do not seem to be the same shape as the theoretical curves may be due, at least in part, to the fact that empirical curves represent averages over a variety of problems of the same type which differ in difficulty, and over the performance of different monkeys which differed significantly. The average of a set of exponential functions, with different rates, is not itself an exponential function (1). The estimates of parameters are at best rough approximations, but it is encouraging to see that the predictions are at least approximately correct, even despite the irregularities of detail.

Warren also has studied performance of monkeys first trained on an F + C + S problem, then retrained with some of the same cues present but some removed (22). A drop in performance was observed whenever any cue was removed. The relative amount of drop caused by removal of various cues, or combinations of cues, checks well with our observed parameters. For example, color had the greatest effect, more than form and size together, and form was slightly more important than size. The stimuli used in this experiment were quite similar to, though not identical with, those used in the experiment on additivity (21).

Theoretically, removal of part of the type-*b* cues would result in recovery from adaptation of certain type-*c* cues which had been adapted only with respect to the cues removed. The greater the proportion of cues removed, the more invalid cues would recover, and the more performance would drop back toward chance. The

quantitative results are approximately in accord with expectation, though in this case no specific calculations are possible.

The above computations were based, in part, on the assumption that form, color, and shape cues are additive. This means, essentially, that the cues are perceptually independent and have nothing in common. This assumption is supplementary to the main theory, but in this case seems reasonable. The theory has been used as a framework within which to state such special hypotheses, and test them. The test, of course, is of the theory *and* the special hypothesis jointly, and failures of such predictions may be attributed either to failure of the theory or of the special hypothesis. This weakens the power of such experiments to verify the theory. Nevertheless, one must keep in mind the distinction between the basic theory and the special assumptions, lest the theory be prematurely discarded because it is combined with an ill-conceived special assumption. It is by no means sure that color, form, and size are additive cues. Brightness and size are probably not independent cues, since they have the common factor of total flux of light.

A more tenuous perceptual hypothesis has also been tested, using the present theory. The results are interesting, if inconclusive. Warren (**20**) studied the rate of solution of form discriminations as a function of the size of forms used. In each problem the two forms were of equal size, but some problems used small forms and other problems used larger forms. On a $3'' \times 3''$ object, forms were of 0.9, 1.8, 2.7, 3.6, 4.5, and 5.4 sq. in. areas. The larger forms were more rapidly mastered, indicating that larger forms give rise to more valid cues.

The following quantitative hypothesis was considered. Suppose that the weight or number of cues arising from the forms is proportional to the area of the forms, while the weight of cues arising from sources other than the forms is constant. Let I be the weight of cues not arising from the forms. The I cues are type-c, since only the forms are differential in Warren's experiment. The weight of cues arising from the forms is proportional to s, the size of the forms. Some of these form-cues are valid type-b cues, arising from differences between the forms. Other form-based cues are invalid type-c cues, arising from nondifferential characteristics of the forms. Let sB be the weight of the type-b cues arising from forms of size s, and sC be the weight of type-c cues arising from forms of size s. The proportion of type-b cues, when forms are of size s, will be

$$\theta_s = \frac{sB}{sB + sC + I} \qquad [20]$$

according to the hypothesis.

We may estimate θ_s for each size of forms employed, and see whether Equation 20 fits the obtained estimates. Since only the *relative* weights

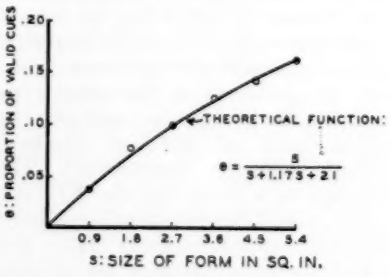

FIG. 3. Proportion of valid cues (estimated from learning rate) as a function of size of forms. The theoretical function is Equation 20 in the text. Data, which served as the basis of estimates of θ, are from Warren (**20**).

of type-*b* and type-*c* cues are important, we may take B as our unit. Equation 20 was fitted to estimates from Warren's data, with $B = 1$, $C = 1.17$, and $I = 21.0$, and the correspondence was found to be close (see Fig. 3).

This test of the theory is considered inconclusive, because the perceptual hypothesis is doubtful. It has been found that the saliency of a form on a background for the monkey depends on whether the form is near the edges of the object or not (**19**; and personal communication from A. J. Riopelle). Since increasing the size of a form which is centered on an object of fixed size brings the form nearer the edge of the object, it seems likely that a more complicated perceptual hypothesis will be needed.

DISCUSSION

This paper has expounded a theory of the formation and operation of "learning sets" in monkeys. Using an earlier theory of discriminative learning as a basis, it has here been assumed that monkeys are capable of responding to abstract, contingent cues. Monkeys, with extended experience, can transcend the concrete stimulus characteristics which ordinarily form the basis of discriminative responses, and respond to conditional cues such as "the object which was correct on the previous trial," etc. These contingent cues are analogous to the cues, whatever they may be, in conditional discriminations—one part of the compound being the characteristics of the object, the other part being previous reinforcements given and perceived by the animal.

This theory emphasized the *cue* value of rewards. Some motivational component of reinforcement is essential, even in the present theory, so that we can define "validity" and "the correct response," but rewards actually must have two effects. One, the traditional reinforcement effect, is as an agent to strengthen certain responses. The other is a cue-value, since food rewards are themselves perceivable cues.

The importance of rewards as cues has been demonstrated in a series of papers by Riopelle and his associates (**14**, **17**, **18**), who have shown that highly efficient performance can be maintained even when no reward is given on the first trial—so long as some informational cue, such as a marble, is used to indicate which object will be reinforced. In the present terminology, they have shown that type-*a* cues, which are usually contingent on previous rewards, can as well be contingent on other things, such as marbles, which have the same place in the procedure as rewards. It is, of course, doubtful that monkeys would long perform effectively if no rewards at all are given for correct responses, since the reinforcing (strengthening) property of rewards is essential. But on Trial 1, in a problem with a sophisticated animal, the reward seems to function mainly as information, and one signal is about as good as another. In fact, since raisins seem to capture the animal's whole attention and detract from observation of the object, it was found that the marble signal was sometimes more efficient than the reward.

It should be evident that the basic concepts of the present formulation are not original with the writer, but instead are drawn from the discussions of writers like Harlow, Meyer, Riopelle, and Warren. The purpose of this study has been to develop a quantitative framework for ideas about learning sets which already existed in the literature. In the present formulation these ideas have of course been modified, and the formal relation-

ships, such as the mechanisms of conditioning and adaptation, and the relationship between cue-validity and rates of learning, are essentially those which were stated in previous papers by the present writer. Apparently, despite the changes and additions, there is some correspondence between the present theory and the theoretical interpretations of learning-set experimenters.[6]

The present theory is sharply limited as to the variables considered. The main factors in performance discussed are θ_a, the relative weight of the abstract cues, θ_b, the relative weight of the concrete differential cues, and V_b, the validity of the type-*b* cues. Only very simple perceptual hypotheses were used in testing the theory. The main important factor which appears to be missing is the possible appearance of observing responses. Other important factors are presumably missing, but have not yet been identified.

Tests of the theory are at present limited to a few fairly obvious and simple computations. Detailed analysis of the large body of experimental data, as indicated by the present theory, may be useful in systematizing and clarifying some of the findings already available. At the same time, extension of the theory to formal statements about reversal, oddities, and other such problems is indicated.

It should be noted that the present theory does not "explain" learning sets, in the sense of reducing the observed behavior to some simpler type of performance. Learning-set performance has not, for example, been shown to be merely another example of S-R association, or innate organization. The present theory is descriptive, attempting to write equations which enable one to predict performance in certain experimental settings. The equations do not arise from simple first principles, but are in a real sense *ad hoc*, with the restriction that they reduce to an earlier theory of discrimination learning for a subject's first problem, or for a subject bereft of type-*a* cues.

Two proper criticisms of the present theory may be mentioned here. One is that the whole formulation hinges on the existence of type-*a* cues, but there is no direct evidence that such cues are present. To this the only answer is that the cues are hypothetical, and any hypothesis is acceptable until it is either disproved or replaced by a better one. A deeper criticism would point out that type-*a* cues are not well described. Several crucial questions have been left unanswered: Are such cues innate or acquired, and if acquired, when are they acquired? How, exactly, are such cues to be described, since they seem to defy the usual sort of S-R analysis? Does the existence of type-*a* cues depend on the phylogenetic level, neural or receptor endowment, or "intelligence" of the subject employed? What kind of neural basis would be required? The only consequence of the presence of type-*a* cues, as the theory stands, is their effect on the performance of sophisticated monkeys—but certainly such performance might arise in any of a variety of ways. Answers to such questions can be obtained only by further investigations. It is hoped that the theory in its present form can take a place as one of the tools in the analysis of the mind of the monkey. Whether or not the tool is useful can be decided only by test.

[6] At least, discussions with Harlow, Riopelle, and Warren have indicated that such agreements exist.

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TRIAL DESIGN IN HUMAN EXPERIMENTS[1]

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With notable exceptions such as free operant behavior (12) and some pure phenomenological experiments (7), psychological experiments consist of trials. Trials constitute the "building blocks" out of which experiments are made, and that branch of the science which deals with the arrangement of numbers of trials under differing conditions and over *Ss* is the study of experimental design. It tries to tell us how we can distribute trials or blocks of trials so that we may get the most out of an experiment.

In this paper the author will try to show that attention can profitably be paid to the *trial itself*, to its composition independent of the particular parameters and variables used. Although many or most of our experiments are made up of trials, the trials per se have not been studied for what they are, and the principles of experimental design have not been extended to cover what goes on "inside" the trial. Trials are the "molecules" out of which experiments are constructed, and the trials themselves can be considered as made up of elements which are the critical events—stimuli, responses, and instructions.

The aim of this paper is threefold: (*a*) To bring attention to the study of the design of trials as an important and integral branch of experimental design, to propose that the trial can profitably be studied in its own right as an adjunct to psychological theory and method; (*b*) to propose one possible model, an "event arrangement" model, as being fairly adequate to cover a number of interesting traditional and novel experiments with human *Ss*; (*c*) to show, by example with this model, how models of trial design can be used to generate new experiments, to provide a framework within which we can discuss experimental results, and, specifically, to provide an operational way of describing correspondences between theoretically derived trial types and experimentally obtained similarities and differences in response measures.

If we are correct, trial design could emerge as a methodology particularly concerned with and suited to the needs of psychology and developed for those needs rather than borrowed from other sciences like agriculture. No other science has experiments made up of trials in the same sense as psychology has. In most psychological experiments a trial is not an arbitrary unit. Psychological experiments are sequences of events, and the fundamental nature of the process studied is more contingent upon some aspects of the sequence than others. We may move some things around, and not others. For example, in a simple size-constancy experiment (*Ss* judge the height of different sized objects at a distance and do this over a number of trials), we may trade or eliminate trials or blocks of trials, we may change variables and parameters including instructions, we may run each trial on a different *S*, and still be studying something which we call perception or a "perceptual type" response. However, there is a limit to the smallness of the unit which we can tamper with and still be studying anything like perception. If we stimulate one *S* and get the

[1] This investigation has been aided in part by a grant from the Foundations' Fund for Research in Psychiatry.

response from another independent *S*, or if we put the response *before* the stimulus in each trial, we have changed the whole "quality" or "character" of the experiment, and other changes (notably in instructions) are necessitated.

DEFINITIONS

There are two general groups of characteristics which make one psychological experiment different from another when they are both made up of trials. In the first group we have those *variables* which change from trial to trial and those *parameters* which change from experiment to experiment (experiment = block of trials); and, second, we have the ordering over time of the occurrence of events within the trial, between trials, and between blocks of trials or experiments. Thus, for example, time in the sense of interval belongs in the group of parameters and variables, but the ordering of events over time is a problem of experimental design and is nonparametric by nature. Traditional experimental design is concerned with arrangement effects *between* trials and *between* blocks of trials. The present model of trial design concerns itself with arrangement effects *within* trials.

The usefulness of trial design, like any other methodology, rests upon the definitions of its fundamental concepts. I have previously rendered an analytic proof of the impossibility of a watertight a priori distinction between stimulus and response (11). Complete definitions of the event-elements which make up a trial are impossible without a number of assumptions regarding the relationship between organism and environment. Thus the definition of the components of a trial can never be a purely operational procedure, if by "operational" we mean that the definition can be made before empirical data are obtained.

The distinction which we, as *E*s, make between stimulus and instructions, for example, is never really clear in any particular case (11) and, furthermore, in the case of multiple-event trials it is very difficult at times to state beforehand how many events of a particular sort we are really dealing with. As we shall see, however, the purposes of trial design do not really demand that we be able to give precise definitions of the components—only that we be able to distinguish them well enough to order them in the trial.

Trials. Trials are unit parts of experiments which are blocks of trials. A trial is the smallest consecutive sequence of event-elements which is repeatable and which contains at least one of each of the critical event-elements (stimulus, instruction, and response). A trial cannot be broken down any further or otherwise altered without altering the character of the experiment, i.e., without changing a perceptual type of experiment into a learning experiment.

The event-elements within a trial *refer to* each other more than they do to other elements outside the trial. Two event-elements are said to refer to each other (and hence belong in the same trial) provided a main effect of the experiment would be discovered by observing the relationship between those events. Thus, in an ISR trial the stimulus and the response within the trial are said to refer to each other, since the relationship between these two events is a main effect of a perception experiment, whereas the relationship between, say, the stimulus of this trial and the response of the preceding trial is not a perceptual effect.

Reference is not the same thing as cause and effect, however, since a response may intimately refer to a stimulus yet to come and may, therefore, be neither the cause nor the effect of that stimulus. In this case the response is

a predictive response and might, according to some theory, be "caused" by a previous stimulus not a part of the present trial.

Events Which Compose Trials. Trial design concerns itself with the composition of the trial. In the present model composition means the combination and permutation of event-elements. The critical events which make up a trial are, according to the model, stimuli (S's), responses (R's), and instructions (I's). Although it may be an advantage in psychological theory to consider instructions as no different from stimuli, in designing trials there is great advantage in considering them as having properties somewhat different from stimuli.

S, **R**, and **I** are defined as critical changes in what we know to be ongoing processes. The notion of "change" or differential is necessary since the model is an event-arrangement model; and if an event is continuous throughout a trial or an experiment, it cannot be arranged or moved around. Otherwise, the use of **S**, **R**, and **I** does not differ in any important way from many of the most common usages in the literature.

Stimulus. **S** is a change within a trial in the stimulating conditions. It is important that reinforcements in learning trials are always S's, as are perceptual and auditory displays, test items, etc. **S** may be defined in either psychological or physical terms depending upon the demands of the situation or the bias of the investigator.

Response. We know that the organism is always responding in some sense or other. Certain changes or rates of change in the **R** process are interesting and relevant, whereas others are not. An **R** is a critical differential in the output of the organism during a trial.

Instruction. I's are a kind of "conditioning" or super-determining stimuli which have the psychological property of orienting or setting the organism for R's (or sometimes for S's) to come. **I**'s refer to and must precede **R**'s, since they must be not incapable of altering the **R** to which they refer. However, R's may precede S's, in which case they are predictive R's. I's are often assumed to be unequivocal, often their physical properties are more trivial than S's, and often they are given in written and spoken words. None of these last characteristics is necessary, however. Reinforcements are never considered as I's—even when verbally delivered. Hence the phrase "You have done very well," is, according to the model, an **S**.

THE EVENT-ARRANGEMENT MODEL

The event-arrangement model works in the following way: by combining and permuting the elements **S**, **R**, and **I**, and adhering to the restrictions, trials of varying complexity can be generated. The model provides a way of breaking down complex trials into simpler trials and of building up ideal trials out of component trials. All of this is done without directly considering the variables of the experiments. Consideration for variables like time can be introduced later. By deducing *all* of the possible trial varieties central to a particular problem, issues take on a new light. Trials are thought of as controls for one another, and answers to questions are possible.

If it is difficult for the reader to consider trials in the abstract without knowing what the variables are, he can be aided by the following device. Imagine a very simple experiment in which the **S** varies in two ways with two values to each way (long, short; red, black); the **R** can be one and only one of these four. The **I** tells the *S* to attend to one of the two aspects (length or color). Now if we run the trials in the order **ISR**, we are studying "perception," and

if **S** varies randomly and **I** is constant ("What length is it?"), we can be sure that any correlation between **S** and **R** over repetitions of the trial is due to "perceptual" accuracy, and we can define perceptual accuracy by that correlation. It is a measure of "how much the *S* sees." If we run the trials **IRS**, we are studying learning, and if **I** is constant ("What length will it be?") and **S** varies in a learnable manner, we can be sure that any within-trial **S**-**R** correlation reflects learning—"how much the *S* has learned." If we run the trials **SIR**, we are studying something conceivably having to do with perception or memory more than learning, but how it differs from **ISR** is not obvious. However, **SIR** suggests that we should vary **I**, and if **S** is constant and **I** varies randomly (sometimes length, sometimes color), we might say that **I**-**R** correlation measures the degree to which the *S* can change set. With some adaptations which are obvious in each case, one can extend this example with its simple variables to the complex trials.

SINGLE EVENT TRIALS

There are six permutations of the three "gross" events **I**, **S**, and **R**. Three of these are not consistent with the restriction that **I**'s must *refer to* an **R** within the same trial and must "set" the organism for that **R**.[2] To do this, **I** must precede **R**.

By elimination we have three consistent single event trials: **ISR**, **IRS**, and **SIR**. These three paradigms are all used fairly frequently in psychological experiments; as a matter of fact, together with the **ISRS** trial discussed later, they practically exhaust the presently explored trial varieties.

The **ISR** paradigm is the typical perception, discrimination, and sensation paradigm. It comes to us from psychophysics and is incorporated in such a diversity of "experiments" as mental tests, projective tests, constancy experiments, absolute and difference threshold experiments, scaling experiments—indeed, all those experiments where the main effects are processes which do not change much with time or experience. There is no feedback in this trial paradigm, the *S* has no knowledge of results within the trial.

The **IRS** paradigm is one learning and concept formation trial used to study those processes which do change with feedback or experience, where we expect learning to occur. In this trial the organism makes an **R** before there is a significant **S** change and then gets feedback in the form of "a reinforcing stimulus" (discovers whether or not he was right, is rewarded or not, punished or not, etc.). This trial is the Humphreys' paradigm trial (5) used by Bush and Mosteller (2) and is the simplest trial type in which learning is a main effect.

The **SIR** trial is very much like the **ISR** trial; in those cases where **S** does not cease before **I** is presented, it may yield identical results in **R**. In the case where there is no overlap in time be-

[2] These "inconsistent" trial types are: **SRI**, **RSI**, and **RIS**. **SRI** is an "unconditioned response" trial, a simple **S**-**R** trial with a useless **I** at the end. The **RSI** trial and the **RIS** trial would be "operant" trials also with useless **I**'s. If we stretch the notion of **I** to include those which "act like" reinforcements (in the model, all reinforcements are considered as **S**'s whether or not they are given verbally), we are tempted to make something out of these trials. However, according to the model, they are not trials because the **I**'s are incapable of altering the **R**'s to which they refer (perhaps the **I**'s "remember it" or "forget it" would be an exception, since they refer backwards) and are, therefore, either not **I**'s as defined by the model or are not a part of the trial to which they are supposed to belong. The model places this restriction on **I**'s and only on **I**'s: **S**'s and **R**'s may come anywhere in a trial, but, if there is an **I** (and there must be at least one per trial), it must precede all **R**'s in that trial.

tween **S** and **I**, however, the differences in **R** observed between these two types of trials may be instructive. These differences are attributable to characteristics of set or attitude, "tuning the organism," etc. Experiments where **I**'s both precede and follow **S**'s have been used to demonstrate the existence of "central perceptual selective processes" as distinguished from "response" tendencies and are important in studying search behavior. In a "perceptual selection" experiment by M. Lawrence [3] (9) **S**'s were presented, each of which contained a lot of various numbers, letters, and symbols. The **S** was a tachistoscopic exposure of a projected slide. In some trials **I**'s to "Look for numbers (letters or symbols)" preceded the brief presentation of the slide, and in some trials the **I**'s "Write down all the numbers you saw" followed the **S**. Since *S*s were better at selecting the correct elements in the **ISR** condition than they were in the **SIR** condition, and since the presentation of the **S**'s was too brief to allow eye movements (search behavior), the difference in results could be attributable to a difference in a central perceptual (selective) readiness process, or to a rapidly declining perceptual "trace." Whether the **ISR**-**SIR** differences are due to forgetting or to the presence of the "state of readiness" could not be distinguished by this simple design.

THE IDEAL TRIALS

The **SIR**-**ISR** difference brings up an important point about the *ideal* trial for any particular experiment. The ideal trial is a kind of a *statement* of an experiment, at the first level, in the shortest possible form. Theoretically, any experiment should consist of an ideal trial and all the controls which are necessary. The necessary controls are completely derivable from the ideal statement by considering all possible sequences within the ideal trial which obey the rule: at least one of each element must be present in a sequence before it can be considered a trial.[4]

Thus the ideal trial for the M. Lawrence-type experiment for studying the effect of *change in position of* **I**'s *relative to* **S** is **ISIR**. For studying the *position of the* **S**'s *relative to* **I** it would be **SISR**. From either of these two ideal trials we may derive the two controls of **SIR** and **ISR**.

The ***ISIR*** *trial.* In actual practice, this trial would at times (probably half the time) demand *S*'s switching his set before making a response: "Look for numbers"—exposure of **S**—"Write down letters"—**R**. Some of the effects might wash out under these conditions, since *S* would learn not to trust *E*. In any case, before the paradigm has been run in its ideal form, we remain ignorant of much concerning this process.

The ***SISR*** *trial.* The fact that this ideal trial is possible shows that there are really two alternative theoretical sets contained in the **I**'s in this experiment, one referring forward and one referring backward to the **S**. As long as the trials **SIR** and **ISR** are run separately, this fact is obscured. For the **SISR** trial to make sense, I must explicitly state to which **S** the **R** is supposed to refer. In practice, this trial would sound like: (*a*) exposure of **S**—"Write down the numbers you have seen (ignoring the next **S**)"—second exposure—**R**; or (*b*) exposure of **S**—

[3] Not to be confused with an experiment by Douglas Lawrence and George Coles (8), who used the same trial types but employed a *recognition* procedure, and hence a very simple stimulus, and found no differences between **ISR** and **SIR**.

[4] Where differences between events of the same class are not considered, $C = (2^r - 1)(2^i - 1)(2^r - 1) - 1$, where C = number of control trials and *s*, *i*, and *r* are the number of **S**'s, **I**'s, and **R**'s in the ideal trial.

"Write down the numbers you are about to see (ignoring the last **S**)"—second exposure—**R**.

The analysis of an experiment, in this way, forces us to discover many of it potentialities and limitations. The "either-or" characteristic of this paradigm, as we have developed it, is evident. That is, the **R** is restricted at any time to one and only one part of one and only one **S**, which must be considered as a limitation. It is a study of the ability of *S* to follow singular and exclusive directions, which come at one of two positions in the trial and which instruct him to attend to one of several aspects of one of two **S**'s occurring at different positions. In the form deduced from the model it is a much more complete and exhaustive experiment and tells us much more about the *process* of selective set.

The complete experiments of two ideal trials and two control trials can answer the following questions, among others:

1. If *S*s are better on **ISR** than on **SIR**, then there exists a central selective state of readiness or a forgetting process or both. (These cannot be distinguished by studying just these two trials separately.)

2. If, in **ISIR**, *S*s are better when the first **I** is the same in content as the second **I** (e.g., both are "numbers") than when the **I**'s are different, this is not due to a declining trace (although, of course, there may be a declining trace which would best be studied by varying time as a parameter). It may be due to the fact that the same **I** is being repeated and thus remembered better. This can be checked by comparison with **ISR** where there is no repetition.

3. If *S*s are no better, in **ISIR**, when the **I**'s are the same than when they are different, this may be due to one of two causes. First, effects may wash out due to the inclusion of multiple **I**'s (*S*s may not pay attention to the first **I** after a while); this can be ascertained by comparison with **SIR** and **ISR**. Second, if there is no perceptual state of readiness operating, the main effects are due to a declining trace.

4. The **SISR** trial enables us to see whether it is where the **I** occurs or in which direction it *refers* which is critical. When **I** refers to the first **S**, results should be identical with **SIR**. When **I** refers to the second **S**, results should be identical with **ISR**. Any departures which obtain are due to the presence of the "uninstructed" **S**.

MULTIPLE EVENT TRIALS

We will now turn to some trials which can be generated by the introduction of more than one of each of the elements **S**, **I**, and **R**.

An example of a multiple ***R*** *trial.* A recent study by Eriksen (3) is a good example of the appropriate use of a multiple **R** trial. *S*s were presented with a size-judgment task under the method of absolute judgment. Instead of using only one **R** or judgment to each **S** presentation, *E* varied the number and nature of the judgmental **R**'s. *S*s were required first to make a verbal absolute judgment, then to move a lever to a distance proportionate to the size of the presented **S**, and then to make a "second guess" absolute judgment, verbally. The experiment was designed to find out whether there was an independent relation between each of the several **R** systems and the **S**, or whether all of the **R**'s would function together. It might be that the first verbal judgment would serve to define the perception for the *S*, the lever **R** and the second verbal judgment being mere reflections of the first judgment without any independent relation to the **S**. Phrased statistically, the question becomes one of determining whether there exist significant par-

tial correlations over trials between each of the **R** systems and the **S** when the remaining **R** systems are held constant. The results showed that the first and second verbal **R**'s and the movement **R** each had a significant correlation with the **S**'s when either or both of the other **R** systems were partialed out and that there was some independent error in each of the **R**'s, since all of the **R** systems together gave a better picture of the **S** than any two. Considering that the second verbal judgment was the same as the first, except for the fact that it came after the first and after the lever-moving judgment, these results are, in a way, surprising. Eriksen concludes:

> One factor that might constitute at least part of this noncorrelated error is a difference in judgmental task that confronts the *O* at the time of the second verbal judgment. For the first judgment *O* must attempt to deal with a continuum of size and to place the stimulus in one of 11 categories along this continuum. However, at the end of the second judgment *O* is faced with essentially a two-category judgment. He has already located the stimulus along the size dimension and his judgment now is in terms of whether the stimulus is apt to be larger or smaller than his first judgment. This difference in judgmental task may be one of the factors subsumed under the noncorrelated error term (3, p. 358).

Eriksen is tentatively proposing the possibility of the discovery of a very interesting effect: the way in which a complex **R** is set up and distributed over time is a condition of the accuracy. We might interpret Eriksen's suggestion as that of the presence of something like a "scanning mechanism." The *S* does better if the **R** is distributed in such a way that he can narrow his search in a systematic fashion—first a gross **R**, then a finer adjustment, etc.

The event-arrangement model can be used to generate a set of controls for testing the existence of a "contingent-**R**" mechanism suggested by Eriksen. The basic experimental trial is a double **R** trial much like Eriksen's, except that we have eliminated the second or "lever" **R** and replaced it with "intervening activity." We also force the *S* to do what Eriksen suspects he is doing on his second **R**, that is, to "correct" his first **R**. The *S* always makes his second **R** using his first **R** as a baseline.

The **R**'s in this experiment differ with regard to the number of alternatives potential in each. Furthermore, any **R** may be present or absent at two places relative to the intervening activity. We are thus free to combine our two **R**'s into a single **R** (equal, in this case, to the product of the alternatives in the separate **R**'s), and to run single and double **R** trials for each of the different **R**'s.

It is believed that this paradigm contains a set of controls to account for possibilities not inherent in Eriksen's design. A plan for the experiment is given in Table 1.

Not all of the controls are absolutely essential to show the existence of "something interesting," but, if the results of this kind of experiment produced any

TABLE 1

DESIGN FOR A PERCEPTION EXPERIMENT *

	1st Time Period	2nd Time Period	3rd Time Period	4th Time Period
1. Ideal trial type	S(X)	R(A)	r	R(B)
2. Control trial type	S(X)	O	r	R(AB)
3. Control trial type	S(X)	R(A)	r	O
4. Control trial type	S(X)	O	r	R(B)
5. Control trial type	S(X)	R(A)	r	R(A)
6. Control trial type	S(X)	R(B)	r	R(B)
7. Control trial type	S(X)	$R(A^2)$	r	O
8. Control trial type	S(X)	O	r	$R(B^2)$
And the transpose of each of the above except 5 and 6	S(X)	R(B)	r	R(A)

* S(X) = a stimulus presentation of one of X possibilities where X is, for example, $\gg$A; R(A) = a response of one of A possibilities; R(B) = a response of one of B possibilities where B is significantly <A; R(AB) = a response of one of A X B possibilities; r = intervening activity; O = no activity.

consistent hierarchy and equivalence of the trial types in terms of subjective accuracy, it would enable us to go a long way in specifying the type of mechanism involved in the handling of incoming information. It might turn out, for example, that any split-up of the **R** is better than no split-up at all; on the other hand, it is possible that only a split-up in which there are significantly fewer (or perhaps even more) categories in the second **R** than in the first **R** will yield a higher correlation with the **S**. We already know that *S*s do better when they have more **R**-alternatives in which to express themselves. If no other substantial difference is obtained between these trial types in the correlation with the **R**, then we can propose that, in the particular situation investigated, this multiple **IR** trial can be reduced to a single **IR** trial, and many problems, including questions of the appropriate definition of **S**, **I**, and **R**, will be greatly simplified, if not solved.

On a different but related topic, Postman has said:

> . . . it appears that an inference concerning perceptual discrimination can be made only if it is based on *at least* two classes of responses. Thus indices of discrimination based on the analysis of subjects' reproductions should be validated by measures derived from other procedures such as the method of recognition. Similarly, thresholds determined from distribution of verbal responses may be supplemented by measures based on matching responses, and so on. Holding the manipulations of the stimulus and the tuning of the organism constant, we measure variations in at least two classes of restricted responses. To the extent that the two (or more) methods of measurement lead to consistent conclusions concerning the subject's discrimination we have identified a *perceptual response disposition*. The larger the number of response classes which we use in our tests of discrimination, the more secure are we in our inference of perceptual response dispositions. It is not to be expected that two or more classes of responses governed by the same disposition will yield identical indices of discriminations, such as thresholds or constant errors. If the indices differ, the analytic problem is to divide the observed systematic variance in responses into two major components: that due to the common perceptual disposition and that contributed by the availability or probability of the specific responses used (**10**, p. 65).

The multiple **IR** trial looks promising as an instrument for getting the kind of data which Postman feels is necessary to define a perceptual response disposition.

The $\mathbf{I_1R_1I_2R_2 \ldots I_nR_nS}$ *trial.* Eriksen's experiment and Postman's statement suggest a wide application for that type of trial where a number of R's, each with its own **I**, follow the **S**. Basically, this trial is still a perceptual trial, since R's, even though there are a great many of them, are still made in the presence of (or following presentation of) the **S**. Two further possibilities immediately arise. First, would there be any advantage to the multiple **IR** series in the learning trial? A trial might then consist of having the *S* predict, in a variety of modes and/or the same mode distributed in various ways, what he expects the **S** to be. An $\mathbf{I_1R_1I_2R_2 \ldots I_nR_nS}$ trial would allow us to study learning under those conditions where there may be a lot of **R** error which does not affect the learning process as far as the *S* is concerned (nonperceptual error), but which otherwise would get in the way of our understanding of the learning process itself. The author wishes to make this suggestion very tentatively, however, since a number of problems arise which are too difficult for consideration here. Instead we shall move on to the second possibility, a multiple **IR** trial which combines the

potential for learning and perception, or discrimination, in the same trial.

*The **IRSR** trial.* The **IRSR** trial is one in which the *S* is asked to make both a predictive or learning **R** and an antecedent, perceptual or discrimination **R** to the same **S**. Where there is **S-R** error in both of these systems, the trial paradigm is not trivial. From the point of view of learning, for example, we can ask the question: What happens to learning when the reinforcement is unclear (when there is error in the discrimination of the reinforcing **S**)? The two- or three-choice learning paradigm (2), in which the *S* is asked to predict whether the **S** is going to be, say, red or black, can now be extended to those situations where the *S* is also unclear as to the outcome of his prediction, but where the *E* has an **S-R** measure of this lack of clarity. We tend to think of reinforcing **S**'s as varying in strength, but they also vary in discriminability, and this is an important characteristic to be studied.

The two control trials which can be derived from the **IRSR** trial are **IRS** and **ISR**. A simple example of the use of these three trials to study the relationship between perception and learning might be the following one, in which only two values of **S** are studied at one time. Let us say that, in any trial, **S** may have one of two values; that is, it may be big or little, the *S* being asked to state (or predict, depending on the trial used) which of the two values has been (or will be) shown to him. Throughout the experiment one value of **S** occurs at random more frequently than the other from trial to trial, so that there will be something for the *S* to learn.

First the **ISR** trial is studied, and a psychometric function can be derived where the difference between the two values of **S** is the variable. We will then know what percentage of the time the *S* can be expected to distinguish between the two **S**'s in an "absolute judgment" sense, given the physical values of the two **S**'s. We then run the **IRS** trial, asking the *S* to predict which of the two values *will* occur (big or little), and we do this at selected points on the psychometric function—for example, where the *S* can distinguish 90%, 50%, and 10%. Of course what the *S* can be expected to "learn" is that one of the values of **S** is more frequent. We can then run the combined **IRSR** trial to see if there is any difference when it is run as a whole.

Such an experiment can be expected to yield important consequences for studying the relationship between perception and learning. Can learning occur when discrimination is very low, if there are enough trials? Does the nature of the process shift as the reward becomes unclear? Must an event be highly distinguishable for it to act as a reinforcement?

From the point of view of perception, the **IRSR** trial seems to fit current perception theory much better than the traditional **ISR** trial. Current perception theory stresses such factors as set, directive state, assumptions, expectancies, determining tendencies, etc., all of which are characteristic of the organism before stimulation. The traditional paradigm allows us to vary these "set" factors only as parameters which can be changed from experiment to experiment, usually through **I**'s. The **IRSR** trial gives us an **R** measure (the first **R**) which corresponds very closely to set, and an opportunity to study changes in set which may occur from trial to trial. Recently there have been suggestions in the perception literature (1, 4) that "responses in the absence of stimuli" will decide nativism-empiricism arguments.

Two interesting correlations may evidence themselves in data collected with the **IRSR** trial which cannot be ob-

tained with simpler perceptual or learning trials. The first of these is the correlation between the two R's within the trial. The question can be asked: "How much does an individual's directive state, set, or predictive **R** tend to determine his perceptual **R** in the case where the other correlations between the two **R**'s and the **S** are zero, or partialed out?" This is the familiar question of the influence of set, which bears a great deal of similarity to the problem of projection. It is also the same as the general question of how much perception is determined by what the organism brings to the situation, as opposed to critical **S** factors. It is traditional in psychological experiments to study the influence of set and motivation factors by varying overall **I**'s and by varying differences between individuals by selection procedures. It is conceivable that changes in set occur from trial to trial due to the experiences of the *S*, as well as from experiment to experiment due to alterations in **I**'s and the like. An **R** measure of set is necessary in order to discover these phenomena if they exist.

The ***ISRS*** *trial.* The **ISRS** trial has the same control trials (**ISR**, **IRS**) as the **IRSR** trial and therefore can be considered both from a perceptual standpoint and from a learning standpoint; either as a basically perceptual trial with an added **S** at the end or as a learning trial with a preceding conditional **S**. Considered as a learning trial, it is the paradigm of the paired associate or Thorndikian (13) experiment; as a psychophysical trial, it provides the *S* with "knowledge-of-results." Those cases where there are large and consistent errors in the beginning of a run—where knowledge of results in the form of an appended **S** makes a difference, in the sense that the *S*'s R's become more accurate over trials—are properly considered as studies of learnable skills. In many cases, notably in psychophysics, knowledge of results does not make much difference. Here we tend to feel that we are studying sensation or a perceptual process with little consistent error, in which learning either plays no part or will play no further part, the process having reached a steady state.

Some very interesting perception experiments have been done using this sort of knowledge-of-results trial. The Ames Demonstrations (6) are presented in such a manner that the *S* is first stimulated by a retinal image leading to a perceptual **R**, which **R**, although it has a strong feeling of surety attached to it, turns out upon further experience by the *S* to have very little correlation with the physical **S**. *Ss* register a high degree of surprise under these circumstances, and this experiment is very upsetting for some of them. With a trial such as this, one would expect that learning might take place with repeated presentations. If learning can be shown to take place under these circumstances, it is very important for perceptual theory, since what we are dealing with here is a very stable percept of which the *S* is extremely sure and which subjectively feels as though it is highly dependent upon the physical **S**. If it can be shown that through repeated presentations of this trial a change can occur in that percept, holding other things constant, then questions regarding the extreme adaptability of the perceptual process are automatically raised. Kilpatrick (6) has attempted to show that such learning does indeed take place. His *Ss* were presented with a room which looked normal, but which was actually distorted in such a way that the retinal image was almost identical to that produced by a normal room. The **R** of the *S* was a motor one in which he poked around the room with a stick or tossed balls at spots of light projected on a wall of the room. These

actions led to a feedback of a sort very unexpected and discomforting to the *S*. In observing changes in the *S*'s perception of the shape of the room, Kilpatrick concludes that there are two processes: first, a change in the relative weights given to the cues, and second, a complete reinterpretation and reorganization of the cues themselves, so that, when now presented with a similar normal room, the *S*s will see it as distorted. The **ISRS** trial is an important tool in the study of the relationship between learning and perception, since, like the **IRSR** trial, it provides opportunities to study change in behavior over trials (learning) in what sometimes is considered an unchanging (perceptual) process.

SUMMARY

A model involving the combination and permutation of the occurrence of events within the experimental trial was suggested which enables one to generate a variety of trial types. The events which make up a trial were proposed as stimuli, instructions, and responses. The definitions of these events were adequate enough to permit the making of certain restrictions on the occurrence of events so that a number of trials, some the same as and some different from those already in use, could be described and their potentialities discussed. The values of the study of trial design lie in its use as an aid in the conceptualization of experiments, particularly in the generation of new experiments and in the providing of a framework within which we can fit experimental findings.

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ON PSYCHOLOGICAL SIMILARITY

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When Hume spoke of composite ideas, such as *table* or *chair,* as being compounded from simple ideas, such as *brown* or *hard,* he found himself facing the question of how one simple idea could resemble a second one more than a third one. He could handle the question of similarity between composite ideas easily enough; the degree of similarity of two composite ideas depended on the number of simple ideas they had in common. But what about similarity between simple ideas themselves? What does it mean to say blue is more similar to green than to red? Hume could not answer this beyond suggesting that similarity between simple ideas is somehow given to us directly—a capitulation that could never satisfy a British empiricist (1; 11, pp. 585–689).

Let us take Hume's predicament as our starting point. Whence does his problem arise? Its origin lies in two omissions: a failure to note the full range of attributes in terms of which objects and events can be grouped and a failure to separate the question of *potential* similarity from that of *psychological* similarity. Consider the first matter. What are the attributes in terms of which objects and events can be grouped or separated? Hume limited himself here—for a reason noted below—to considering simple sensory attributes displayed by the object itself—for example, the brownness or hardness of a table. But a table can be defined in terms of many more attributes than these. To mention but a few possibilities, it can be defined in terms of use (the things done upon it), of location (the places where it is found), or of construction (how it is built). Such attributes as these are no less reducible to a sensory basis than the ones mentioned by Hume, and they suggest a possible answer to Hume's question. We may say green seems more similar to blue than to red because there indeed *are* more attribute-values that green and blue but not red have in common, while there are fewer that green and red but not blue have in common. Hume may, that is to say, be able to retain his common attribute view of similarity by accepting a wider definition of the term "attribute." Thus, for instance, green and blue—but not red—have in common the attribute-value of being colors of grass in the country, colors of water and hence cool colors, summer landscape colors, and so on.

But we have not fully answered the question yet. Some people may *not* say that green and blue are more similar than green and red. The attributive basis for a judgment of similarity may be present and yet the judgment not be made; and, on the other hand, an attributive basis for such a judgment may be lacking and yet the items may be judged similar. There would seem to be a difference, then, between potential similarity and psychological similarity. Part of Hume's difficulty seems to arise from equating the two. This leads him to fill the perceiver's head

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with similacral (i.e., photographic) representations of environmental attribute-values, and hence to ignore the possibility that the observer may perform a selecting and ignoring function with regard to attribute-values which are environmentally available. Indeed, it is this view that leads him to the first omission we noted—his failure to specify the full range of environmental attributes. For insofar as one assumes the organism to register in similacral fashion what the environment offers, one must conceive of that environment as relatively meager in its offerings, or else the organism would be swamped with information. If, however, one conceives of the organism as selecting some attributes by which to compare items and ignoring other attributes, then it becomes reasonable to suggest a difference between potential and psychological similarity. Potential similarity may well be most usefully measured in terms of the number of common environmental features that two objects or events are found to display—using the broad definition of attribute noted above, and determining the commonalities by reliable means.[2]

This definition has its basis in the old view of similarity as common stimulus elements, to be sure—but it is very different from that view, since the common-stimulus-elements approach used the narrow definition of attribute for which we criticized Hume, and that view was thought to apply to psychological similarity, which we shall see is often not the case. Establishment of the degree of potential similarity between two things provides us with an important baseline: It lets us validate or contravalidate individual reports of similarity. A person may, for instance, report that two problems are similar in the sense that the same solution is thought to be applicable to both; to ask whether this feeling of similarity is valid is to ask whether the same solution is in fact applicable to both problems; are they potentially similar in the sense of having this property in common? Or a person may report that two objects are similar in the sense that both are thought usable for driving a nail into a board. To validate or contravalidate this impression of similarity, we determine whether both objects can in fact be used this way.

Broadening the definition of "attribute" and recognizing that potential and psychological similarity are distinct but related issues, suggest that the question of psychological similarity may lie at the basis of diverse kinds of psychological research on cognition that have hitherto usually been considered apart from each other—ranging from studies on stimulus generalization to studies on learning and thinking. We submit it may be fruitful to inquire how some of this research bears on the issues we have raised, and this for two reasons. First of all, we have not yet examined the possible ways in which psychological similarity might be defined, although we have suggested what seems to us a tenable definition for potential similarity. Insofar as this research really bears on psychological similarity, it may help us weigh alternative definitions of the term. Secondly, such exploration of definitions may well have experimental implications for further research in these diverse areas.

SOME DEFINITIONS OF PSYCHOLOGICAL SIMILARITY

One may suggest at least four ways of defining psychological similarity. The first derives from Hume's approach.

[2] The problem of just what means to use and how, has been treated at length by Brunswik (3) in his discussions of ecological validity. The means are, basically, responses of a sample of judges—and they may be responses as varied as reading a light meter or judging a painting's colors to be warm or cool. The validation, then, is consensual in nature.

Hume, in equating potential and psychological similarity, supposes that if common properties are present in the environment, they will be perceived by the person. Hence, impressions of similarity can be directly controlled by varying the environment's potential similarity in the sense of the common values of attributes it displays. The selecting and ignoring capacities of the organism are neglected, and psychological similarity is defined in terms of *common environmental properties*. An alternative approach is to recognize the organism's selective functions, and suggest that recognition of common environmental properties depends upon making a common response to instances that share this attribute-value. The attribute-value may be environmentally present in two items and yet the person may not offer a common response to them; or the attribute-value may be absent and yet a common response be made. There is not necessarily a direct relation between psychological and potential similarity. Psychological similarity here, then, is defined in terms of *common responses*. Yet another possible approach is to suggest there are neural traces laid down along various dimensions when a stimulus impinges, and psychological similarity depends on how far a new stimulus is from the old stimulus on such a dimension—the view of *primary stimulation gradients*.

A fourth view may also be proposed. It is related, to be sure, to the view of common responses, but seeks to separate the process of recognizing similarity from making a common response. Yet this fourth approach is not that of stimulation gradients, for it does not assume that there is necessarily an inverse relationship between degree of recognized similarity and distance of new from old stimuli along some dimension. The position may be put somewhat as follows. Although a common response may be forthcoming to signal the recognition of similarity when two events occur, this response is itself a rather trivial matter (it may be made with the big toe as well as with the vocal cords, and need not be of fixed character), and its being made depends on the prior learning of a rule for categorizing events as equivalent. This rule specifies what property (attribute-value) or properties must be conjointly found in order to assign events to the same class, and also tells us the nature of the contrast being made—the relevant alternative class whose members are to be judged non-exemplars of the class of interest.[3] We construct this contrast class in such ways as the following. (*a*) By implication, from the degree of complexity of the class of interest; since the more complex that class is, the more likely we are to consider small deviations from it as the only relevant alternative class or classes, and to totally neglect large deviations.[4] Thus, for instance, we are likely to contrast cocker spaniels with setters or poodles and not with all other animals, whereas "all other animals" would be a relevant contrast class if we were interested in dogs rather than cocker spaniels. (*b*) By experience with all the instances, positive and negative, to which we are exposed, for these establish the domain of material relevant to us. Construction of a contrast class in turn suggests which properties we may best use for discriminating positive from relevant negative instances. For example, while the attribute of size will not let us discriminate dogs from all other animals, it does suffice to separate setters from cocker spaniels.

The classification rule that we learn, then, has two aspects: it tells us on

[3] See Bruner, Goodnow, and Austin (**2**), p. 38; and Kelley (**13**), pp. 303–305.

[4] The "implication" in question here is not one of logical necessity, but rather a strategy that people tend to use—and indeed a valuable one—for determining the level of classificategory discriminations to make in particular situations.

what basis we are to group two events in one class and also tells us the range of other events with which we are contrasting positive instances of the class of interest—it tells us what negative instances are relevant. The rule serves to delimit a particular domain of events, and to draw a *contrast* between two subsets within that domain: positive and negative instances of the class in question. This rule stipulates that certain properties must be found compresent, i.e., together, in order to place an instance in a given class. Such a rule for class-inclusion becomes harder to attain, the greater the number of properties which the rule requires to be compresent before inclusion in the given class is warranted. This is the case because the larger the number of properties whose compresence is required in this way for class-inclusion, the greater is the number of properties which are found compresent in at least some of the relevant negative instances as well as in all positive instances of the class in question, thus making it more difficult to locate the difference between positive and negative instances.

In brief, then, this fourth proposal suggests that recognition of similarity depends on applying a rule which leads one to *assign items to a common category*. There is much more to similarity than the response of making this assignment. For prior to that, one must learn the rule which guides the response.[5]

Each of these ways of defining psychological similarity has much to be said in its favor, but no one of them seems able to account for all the evidence. Our aim in the following pages is to suggest some possible limitations which various of these views face, and to sketch the possible fruitfulness of exploring more fully the one of these approaches that defines psychological similarity in terms of assigning events to a common category.

If, as we have suggested, diverse areas of psychological research all concern psychological similarity, one is led to ask whether these areas may be using the same kind of procedure, since a common concern sometimes intimates a common procedure of inquiry. This seems indeed to be the case here, and the procedure in question is that of transfer—the influence of experience in one situation on the handling of a subsequent situation. Insofar as something learned in a first situation will aid performance in a second one, then the subject will show positive transfer if he recognizes the two situations to be similar; while insofar as something learned in that first situation will hinder performance in the second, then he will show negative transfer if he recognizes the two situations as similar. Transferability of learning thus provides a set of operations for determining whether recognition of similarity occurred. As we turn to various experiments now, in further considering the definitions of psychological similarity that we have reviewed, we shall find that all of them in effect use this transferability procedure.

COMMON ENVIRONMENTAL PROPERTIES

Since this is, perhaps, the most familiar and earliest view as to the nature of psychological similarity, we need not dwell on the many kinds of evidence from which it arose—for instance, motor learning studies, of which the work by Lewis, McAllister, and Adams (20,

[5] If one prefers to argue that such a rule is simply a complex response, it is incumbent upon one to specify the responses constituting that rule. The suggestion often made at this juncture is that the rule is no more than the verbal responses which express it. But in animal learning studies discussed later—where one cannot, therefore, say the rule is nothing more than verbal responses—we shall note that the common responses which are present do not in fact constitute the basis of the rule that is learned.

21) provides a recent example. Commonality of the environmental properties is established by the identity of measurement readings of the centimeters-grams-seconds variety in the two situations being compared (call them Original Learning, OL, and Transfer Learning, TL). The evidence indicates that frequently, to be sure, transferability of learning, and hence psychological similarity, is related to presence of common environmental properties in these two situations. But there are, on the other hand, cases where presentation of common properties in OL and TL does not result in psychological similarity of the two and hence in greater transfer.

Consider, for instance, a study by Schwarz (27). In the OL task, the subject learned to attach a certain response to a certain stimulus. In the TL task, he had to learn to link a different response to that old stimulus. If transfer here depends on common environmental properties, one would expect associative interference—that is, the old stimulus should tend to elicit the old response; it should be transferred to the TL task despite its inappropriateness there. Actually, such associative interference occurred only if the subject was forced to pay attention to something other than this particular task; if allowed to attend to this task, the old response was not transferred despite identity of stimulus. When the subject was "paying attention" he was grouping environmental events differently than when he was "not paying attention." When not paying attention, stimulus identity was allowed to become the attributive basis for classification; but when paying attention, the temporal difference between "stimulus in the OL task" and "stimulus in the TL task" was effective in directing the subject to assign the OL and TL stimuli to different classes based on this temporal distinction, even though they were physically identical. Assigning them to different classes meant that negative transfer was avoided.

Or consider a study by Ellis (4) in which college students learned finger mazes. Three different mazes were used in all, Maze 3 consisting of Mazes 1 and 2 linked together. One group learned Mazes 1 and 3, another learned Mazes 2 and 3, and a third learned Maze 3 alone. Whereas an identical-stimulus-elements approach would predict marked positive transfer for the first two groups in their learning of Maze 3, in actuality those two groups were not significantly better in their learning of Maze 3 than the group learning that maze *de novo*. Objectively identical stimulus elements did not per se lead to recognition of similarity.

Common environmental attributes, then, do not seem to be an infallible indicator of psychological similarity. Such similarity may, under these conditions, be present for the person—but then again it may not.

COMMON RESPONSES

It was perhaps the Kantian revolution in philosophy that first called into question Hume's conception of the organism as a mirror of environmental stimulation, and suggested instead that the organism is a filterer and arranger of environmental events. The "mentalism" of Kant's approach banned it from having direct repercussions in American psychology until it was realized that considering the organism an active mediator does not require descent into its depths, but rather can be achieved by noting how the organism responds to its environment. One need not go beyond the relation between environmental stimuli and a person's responses. If, according to this view, a person was found to respond the same way to two objectively different situations, then those two situations were psychologi-

cally similar for him. And if, on the other hand, a person responded differently despite repetition of the same situation (common environmental properties), then no such psychological similarity obtained. Such an approach constituted a tremendous advance. The Humian assumption of man as capable merely of similacral representations of his environment was overcome, and in its place the capacities of the organism to select, ignore, and emphasize environmental attributes were recognized. The exciting effects of this idea can be seen, for example, in the early work by Shipley (28, 29). In one experiment, linking an eyewink response to both light and shock, where shock elicited finger withdrawal, resulted in light becoming adequate to elicit finger withdrawal too. The common response of winking made to both light and shock indicated their psychological similarity for the subject. Whereas we would say this response was a signal of this similarity, proponents of the common response view would hold that this response itself constitutes that similarity. The implications of this kind of approach are ably developed by Osgood (25), among others, and its applicability to many questions of psychological similarity is clear.

The common-response view leaves us, however, with some uneasiness. For one thing, those who maintain it sometimes seem to rely on the magical efficacy of the term *response* to provide an aura of explanation where none really exists. The premise of the approach is that transfer will occur, and hence two events will seem similar, to the extent to which the organism makes the same response to both. But from here on the going becomes less simple. Paradoxically enough, the insufficiency of this premise seems to be tacitly recognized, for the full view consists of an attempt to defend the premise's lacks. Since the hypothesized common response that supposedly mediates the transfer sometimes cannot be located even though transfer is found, one meets the rationalization that sometimes the common response linked to disparate stimuli is not observable, but in all such cases these unobservable responses are supposed to have developed as fractional anticipations of observable ones. This is an empty-organism theory with a new twist. The organism gets filled eventually, but only with fractions of things that were overt responses once.

It seems relevant to remember that much of the impetus behind this view derives not from considerations of scientific adequacy as such, but rather from the cultural milieu of an earlier behaviorism in which the model of the organism as capable of transformed representations of its environment was still held in disrepute, as "mentalistic." The common response theory hence sought to make this model acceptable to the scientific community by framing it in the language of responses. This end has been accomplished long since, however, so it would perhaps be advisable now to restrict more carefully use of the term "response" to its usual meaning: muscular and glandular reactions. We would note that the restraints imposed by the nature of muscles and glands imply that behavior must be more limited and have fewer dimensions about it than what goes on inside the organism.[6] Why, then, limit on a priori grounds one's conception of what goes on in the organism to fractions of behavior? It does not make constructs more scientific

[6] One way to indicate this difference between behavior and what goes on inside the organism, is in combinatorial terms: there are more elements that can be combined, and hence a greater variety of possible outcomes, at the neuron level, than at the level of muscles and glands.

to frame them in the same terms as observables. Rather, the reason we postulate constructs is that the relations among observables are extremely complex, and it often proves necessary to invest our constructs with properties different from those of behavior in order the better to predict that very behavior. The interests of explanation are not served simply by filling the organism with analogues of behavior. Hospers (9) and other philosophers have well pointed out that such reduction to the familiar is not what we really mean by scientific explanation.

But were our uneasiness over the common-response approach to stem only from general considerations such as these, it might well be dismissed. Dismissal becomes more difficult, however, when one notes the gradual accumulation of experimental evidence that questions the sufficiency of response identity for mediating transfer of learning. Consider, for instance, a recent study by Bruner, Mandler, O'Dowd, and Wallach.[7] Rats were overtrained to run left-right-left-right at successive choice points of a linear maze, and then were faced with the task of learning to run right-left-right-left in the same maze. The object was to find whether there was positive transfer from learning the first turn sequence to learning its mirror-image reversal.

According to the response-mediation approach, transfer is said to depend on identity of response, and the greatest identity of response between OL and TL here is the single-alternation weaving motion whereby the rat alternates from one side to the other at successive choice points. The faster a rat runs from start box to goal box, the more of a weaving motion he learns. Insofar, then, as transfer depends on the identity of a weaving-alternation response in OL and TL, rats running faster should show more positive transfer than rats running more slowly. The results do not support this prediction. There was no correlation between speed of running during OL and degree of positive transfer on TL, within any of the groups run. Furthermore, considering differences in running speed between groups—differences that were created by varying hunger conditions—groups whose drive conditions resulted in their running slowly during either OL or TL or both showed positive transfer, while the group whose drive conditions resulted in its running fast during both OL and TL showed no transfer. Apparently, therefore, something other than a common response of weaving single alternation causes the transfer in the present experiment.

Or take North's finding (23, 24) that the performance of rats on discrimination reversals was no better when the rat was made to correct overtly his mistakes than when mistakes were left uncorrected. It was expected that reversal learning would be better in the former condition, since it permitted a correct response to occur after a wrong choice, and this correct response was supposed to become associated to the present maze stimuli, and hence be evoked by these stimuli on the next trial, thereby mediating a full-fledged overt correct response. Identity of response thus was expected to mediate reversal learning. Much positive transfer in the learning of successive reversals occurred, but it was not due to response mediation. It occurred, rather, only when there was a sufficient amount of overtraining on each reversal—a result also obtained in the study on single alternation cited above. Apparently, for the animal to classify initial and later situations in terms of the more abstract property which they in fact have in

[7] Bruner, J. S., Mandler, Jean M., O'Dowd, D., & Wallach, M. A. "The role of overlearning and drive level in reversal learning." To be published.

common, he must be given enough practice on the former situation so that its more concrete properties no longer require all his attention. The abstract property in question was single alternation in the case of the first study noted; reversal of position response on encountering an error, in the case of the second study. The concrete properties involved were particular sorts of muscular kinesthesis that occur in relation to particular olfactory and visual cues from various parts of the maze.

To be sure, insofar as one broadens the definition of "response" to include anything going on inside the organism, as some theorists such as Osgood (25) and Mandler (22) advise, evidence of the above sorts does not run counter to the common-response view. But broadening the definition in this way would seem more to cloud than to clarify the issues. There are grounds, then, for suggesting that presence or absence of common responses is not an infallible indication of presence or absence of psychological similarity, although there are certainly cases where such responses do serve in this manner.

PRIMARY STIMULATION GRADIENTS

Many studies have been done in support of a view of psychological similarity which may be stated somewhat as follows. When a stimulus impinges on a receptor and becomes associated to a response, a neural gradient is laid down such that other stimuli will also elicit this response but to lesser degree, response elicitability decreasing with increasing distance of the new stimulus from the training stimulus along some dimension of stimulus differences—for instance, intensity or pitch of tones. The dimension of primary stimulus generalization need not be innate, as was assumed by those proponents of this approach who were most directly in the Pavlovian tradition, like Hull (10); but rather may be the product of learned differentiation (i.e., discrimination training)—a coming into closer contact with stimulus differences—as in the manner suggested by Gibson and Gibson (5). But however the dimensions are thought to arise, the position in question assumes the shape of the gradient laid down upon them to fall off on either side of the point of stimulation along these dimensions; and, if training and test stimuli are presented at discriminably separate points along a dimension which the individual has learned to discriminate, or can innately discriminate, then generalization is expected to occur along that dimension.

These ideas of a normal distribution of intensity of excitation about a point of stimulation, and the automatic fashion in which generalization is expected to occur between discriminable points on any discriminable dimension when these points are stimulated in succession, were derived from an earlier era of physiological research in which spatial representations of sensory qualities in the cortex were a prime concern—we may see it in such apparently opposed currents of thought as Pavlov (26) on the one hand, and Köhler (19) on the other. And some work, to be sure, does seem best interpretable in terms of such ideas (e.g., Guttman and Kalish, 8). But there appear to be other studies which, although ostensibly dealing with the same processes, may perhaps be better interpreted in different terms. In a number of experiments on "stimulus generalization" in humans, psychological similarity may be mediated not by that process but rather by the class to which the subject assigns various inputs. The magnitude of the subject's response to a test stimulus can depend, one may suggest, on whether or not he places both training and test stimuli in a common class. There may well be individual differences as to

whether these two stimuli are placed in the same category or not; and in addition, experimental conditions may be set up in such a manner as to induce or discourage the placement of those two stimuli in the same class without varying these stimulus presentations themselves. If such diffuse factors as these were to cause differences in magnitude of response to the test stimulus—despite the facts that the dimension along which the training and test stimuli fall and the differences along that dimension are easily discriminable by the subject, and the training and test stimuli presented are the same for all these subjects—then the sufficiency of a stimulus-generalization explanation for such data would be called into question. And, in fact, there do seem to be cases where differences of these kinds occur.

We may note, for example, a study by Wickens, Schroder, and Snide (32), using the GSR. Adult subjects in one group received shock-reinforced presentations of a particular tone, and then were given extinction trials with one of three tones at varying frequency distances from the training tone. Subjects in another group underwent the same regimen, except they heard a nonreinforced click interspersed among the reinforced tone presentations during training. The group not hearing clicks—qua group—yielded a gradient of decreasing GSR magnitude as distance between training and test stimuli increased; but the height and shape of the gradient differed on different extinction trials, and only by averaging the results of large numbers of subjects (24 in each extinction subgroup) could one tease out even these not very great consistencies. The group hearing clicks showed no clear gradients at all, even as a group.

Why the different results from these two conditions? Recall what we noted earlier concerning the two aspects of rules for classing events as equivalent: that they specify the range of negative instances we are seeking to discriminate, as well as the nature of positive exemplars of the class of interest. We would suggest that presence versus absence of clicks during training resulted in the learning of different rules for the classification of test stimuli during extinction. The group hearing clicks used the property "tones" as definitive for class-inclusion, and excluded clicks. All test tones were assigned to one class. Because of what they experienced during the training series, the domain of instances for this group consisted of "tones" and "clicks." The group not hearing clicks, on the other hand, used the properties "tones, and within such-and-such range of pitch" as definitive for class-inclusion, and excluded tones of other pitch—these latter being the relevant negative exemplars for them. Only some test tones met the specifications for this class. Because, then, this group experienced only the repeated tone during training—i.e., heard no clicks—this group's domain of instances consisted of "tones of various pitches." Hence, we would expect the GSR to the test tones far from the training tone to be much less than that to the test tone closest to the training tone for the group not hearing clicks—and this was in fact the case. These subjects assigned only the nearest test tones to the same class as the training tone, further-away test tones being excluded. But we would expect no clear trends of differences in GSR among the test tones for the group hearing clicks—and this also was the case. These subjects assigned all test tones to the same class as the training tone, clicks being the items which were excluded from the class. The difference between the click and no-click conditions, then, seems to be that the subject was induced to classify the tones differently in each case.

It may be that many other "generalization" experiments also involve a categorization process like that we postulate for the "no-click" group in the above study: Test stimuli close to the training stimulus are placed in one class with the latter, those further from the training stimulus being excluded. The widths of the class in question vary across individuals, and hence one finds a smooth normal-curve type of gradient only when one averages all subjects together. But there are further individual differences in classification, and, what is more, classifications may change as a test series proceeds. Hence one finds, for instance, nonconsistent results over extinction trials taking groups of subjects as wholes—as in a study by Grant and Schiller (7). That these effects are due to categorization differences seems a reasonable hypothesis when one considers work like that by Wickens et al., where, on our interpretation, the probabilities of alternative rules for classification were varied, and this was found to cause differences in magnitude of response to a test stimulus.

Although primary stimulation gradients seem able to account for some cases of psychological similarity, then, there are others in which a different process may well be at work.

ASSIGNMENT TO A COMMON CATEGORY

Each of the three definitions of psychological similarity noted above has been found to have only limited applicability. Such is also the case with this fourth definition. Since, however, it is younger than the others, its implications have not yet been thoroughly explored; and since it seems relevant in many cases where the other definitions cannot easily be applied, it seems advisable to examine it further with regard to its range of utility.

According to this fourth definition, when we have attained a rule which bisects a domain of events into a set that exemplifies a certain property or conjunction of properties and a particular contrasting set that does not, then we will transfer behavior from one to another of two events that fall into the former set, but will tend not to transfer behavior from either of them to an event in the latter set. Again, then, the relation has two aspects: It tells us the range of negative instances which are relevant to our purposes of discrimination, as well as the basis on which we are to declare events positive exemplars of the class of interest. In short, recognition of the similarity of two events depends on their being classed as equivalent—as exemplifying the property or conjunction of properties in question, and as standing in contrast to a particular range of nonexemplars.[8] We learn to take one or more particular properties as criterial for sorting a domain of events.

There would seem to be two main determinants of the kinds of classification rules we attain: the nature of the instances presented (i.e., the environmental properties to which we have been exposed); and our predilections for some bases of classification and prejudices against others. Varying either the nature of the instances or our classification preferences hence should influence psychological similarity in predictable ways.[9] Let us see if this approach seems to contribute anything in considering two psychological issues: the

[8] This view is related to those of Goodman (6) and Kelley (13), and was first approached in Klüver's early work (14).

[9] Since the present paper's aim is to compare this fourth approach to psychological similarity with the other three and to provide initial indications of that approach's empirical utility, we cannot discuss this approach in greater detail than is provided in the present paragraphs and in the earlier section where this view was introduced. More detailed discussion of this view in particular will be provided in a subsequent paper.

first, transposition—a matter around which much empirical study has centered; and the second, distinctive similarity—Köhler's demonstration of the importance of similarity in guiding attempts at more complex human problem-solving.

Transposition

Some years ago, Köhler (**17**, pp. 217–227) and Koffka (**15**) argued that discriminating in terms of such relations as "brighter than" or "larger than" is more primitive, more basic, than discriminating in terms of absolute sensory qualities such as brightnesses and sizes. Spence (**30**) argued just as insistently, however, that responding on the basis of absolute stimulus qualities such as brightness or size is more primitive, and relational responding is a result of the algebraic summation of response tendencies to absolute stimuli. But one may suggest a third alternative. Perhaps higher organisms (and certainly humans) can learn to respond either in terms of relations between stimuli or in terms of absolute stimulus qualities, and which selection they will make depends largely on the conditions instituted concerning classification preferences and the nature of the instances.

Consider, for example, the nature of the instances. We would predict a greater tendency for subjects to respond relationally on a transfer test, when trained on couplets of one large and one small form apiece, and when the particular forms used vary in size (but not shape) across couplets, than if the same amount of training were given on one repeated couplet of a large and small form; in both cases response to the larger form is always rewarded during training. In the former condition, the nature of the instances is such as to permit the subject to rule out the possibility of classifying the data in terms of the absolute stimulus quality of size, and the only class into which all data fit is one defined by the relational property of "larger than"; whereas in the latter condition, the kinds of instances presented permit classification on the basis of absolute size or relative size. We could just as well, of course, arrange our instances so as to rule out the possibility of classifying on the basis of relative size, simply by always rewarding choice of the same particular stimulus form, and having it appear sometimes paired with a larger form, sometimes with a smaller form. In a like manner, whether the subject will classify on an absolute, relational, or even some other basis depends on his biases for or against particular bases of classification, as induced by instructions or other means. Given variation in the nature of the instances and in such biases, it would seem one can create conditions that will induce the subject to classify the training instances, and hence to transfer, on any basis we desire.

Thus, work with children by Jackson and Jerome (**12**), for instance, indicates that how the child categorizes the training instances depends on whether he is asked to decide which stimulus is correct after the presentation of both, or rather is required to indicate, after each stimulus is presented, whether it is the correct or incorrect stimulus. As we would expect from the differing emphases on relational versus absolute classifying implied by these different conditions of instance presentation, relational choices were preponderant when the child gave one over-all response after simultaneous or successive presentation of both stimuli, whereas absolute choices were in the majority when the child had to respond to one stimulus before the second was presented. Requiring the subject to make a separate response to each stimulus favors classifying on an absolute basis, whereas requiring the

subject to make a single response to both stimuli as a pair favors classifying on a relational basis.

Stevenson, Iscoe, and McConnell (31) found that college students varied in the basis they used for responding on the first transposition test trial; many, to be sure, responded on a relational basis, but some responded on an absolute basis. Apparently, different subjects received different impressions about what was required of them, and hence classified the data presented to them in different ways. What seem needed in this area, then, are studies analyzing the conditions of instance presentation and biases concerning particular attributes that influence the basis the subject chooses for classifying the training stimuli—conditions which therefore influence the kind of transposition shown.

Distinctive Similarity

Köhler (16) uses the term "distinctive similarity" for denoting that particular items resemble each other more than they resemble the rest of the situation. The nature of this greater or lesser resemblance is itself, however, left unanalyzed, although an earlier experiment by Köhler and von Restorff (18) had explored the effects of such similarity on complex human problem-solving. Two groups were taught a principle for solving an arithmetic problem, and transfer was measured later to the solution of a new arithmetic problem on which that principle—an arithmetic shortcut—could also be used. Between these two occurrences, one group was given arithmetic problems to which, however, this principle was not applicable, whereas the other group was given entirely different tasks to do—the solution of matchstick puzzles. The latter group showed greater transfer of the principle to the new problem than did the former group. Thus similarity of the first to the last problem, in particular, was greater for the latter group than for the former group; in one case it was "distinctive," in the other case it was not. But of what does this psychological similarity consist?

One may suggest that analysis of the classification tasks facing the subjects in Köhler and von Restorff's groups casts light on something which these authors really left shrouded in mystery—the nature of distinctive similarity. After being shown the application of the principle to the initial arithmetic problem, the task of the subject became, essentially, that of attaining the rule specifying what property or properties must be conjointly found in order to assign problems to the class to which this principle is applicable, and specifying the relevant range of negative instances with which the subject must concern himself. The group performing better on the transfer test learned but a simple rule for classification: only one obvious property made an item a positive exemplar of the class of items to which the shortcut principle was applicable—namely, that the item be an arithmetic problem; and the degree of overlap of positive and relevant negative instances in terms of compresence of properties was minimal—no more than that they were all "problems." The group performing less well on the transfer test was given the task of learning a more complex rule for classification; a conjunction of properties was required to make an item a positive exemplar of the class in question: the item had to be an arithmetic problem *and also* had to exhibit certain additional features. For this group there are more properties compresent for positive and relevant negative instances (e.g., that the task be not only a problem but also an *arithmetic* problem is compresent for both) than for the other group. In the first case, the domain of relevant events is "arithmetic and matchstick prob-

lems," and the contrast is between these two types. In the second case, the domain is "arithmetic problems," and the contrast is between arithmetic problems with certain features and arithmetic problems with other features. Again, the nature of the instances presented to the subject determines the kind of classification attempted. In short, "distinctive similarity" of two events means that the rule for assigning them to the same class provides for a minimum number of properties to be found compresent in at least some of the relevant negative instances, as well as all the positive instances of that class. The difference between positive and relevant negative exemplars is easier to locate.

Could the rule for the group that does poorly be rendered easier by a change in biases of the subjects toward particular attributes? Let us consider the following variation on the Köhler and von Restorff experiment. Suppose we give another group all mathematics problems and in the same order as the original group, but tell them, after the shortcut has been explained, that not all the subsequent problems are amenable to the shortcut, but that some will be. We have thereby set these subjects to remain vigilant throughout the series for the features of later problems that are the same as those in the initial one—the features that hence permit the principle to be applied—and would expect them to apply the shortcut to the last problem more often than did the group not given this set. Without changing the problems, then, we would expect the percentage of shortcut solutions to increase because of this change in set; these subjects have been more sharply tuned to try to find what features of arithmetic problems permit application of the shortcut. They have been encouraged to try to take a particular conjunction of properties as criterial for sorting events.

If one wishes to study distinctive similarity in the transfer of principles for problem-solving, then, the recommendation would seem to be this. Let us determine the conditions of instance presentation and attribute biases that aid or hinder the arrival of the subjects at rules for classifying tasks in terms of properties which they potentially have in common—conditions which aid or hinder, in other words, a match between psychological similarity and potential similarity.

CONCLUSION

The aim of this discussion has been twofold. First, after pointing out the ubiquitousness of the issue of psychological similarity in various areas of research on cognition, to note the way in which a common procedure, that of the transfer experiment, tends to unite them, and to urge that they be conceptualized in an integrated manner. Second, to consider the values and difficulties of four conceptions of psychological similarity, defined in terms of common environmental properties, common responses, primary stimulation gradients, and assignment to a common category. All four definitions are, to be sure, of limited applicability—but the last of them appears to apply in many instances where the first three do not. Several experimental implications of defining psychological similarity in terms of assignment to a common category were explored, and were taken to indicate that further work with this approach may well prove fruitful.

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CONFORMITY, STATUS, AND IDIOSYNCRASY CREDIT[1]

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Something of a paradox exists in the prevailing treatments of conformity and status. Students of social psychology are likely to be left with the pat impression that the freely chosen leader conforms to, and perhaps tenaciously upholds, the norms of his group. Yet this kind of leadership is also presented as a status sufficient to provide latitude for directing and altering group norms (11, p. 416). From their recent experimental work in this area, Dittes and Kelley have voiced a doubt that the relationship between conformity and status is ever a simple one (7, p. 106). The evidence favors their assertion.

Although these phenomena may be treated as discrete entities, they both arise from interaction between an individual and a set of relevant other individuals constituting a group. To say that an individual conforms, or that he has status, is not to say that these are independently determined states nor that they are terminal; they have some common origin in a phenomenal relationship which persists over time. Conformity and status may be thought of therefore as mutually dependent, and transitionally effective upon subsequent interactions. With this as a framework, several general conceptions will be expressed here regarding mechanisms which produce these phenomena and govern their relationship to one another.

In a gross way, three classes of variables, or elements, are necessary to this conceptual scheme: characteristics of the individual himself; characteristics of the group with which he interacts; and outcomes of interaction representing a past history which may alter the relationship of the former elements.

Of particular importance as a mediating process is the changing perception brought about in the individual and the group by their interaction; the third element is, in effect, this process. A distinction is required, therefore, between the phenomenal and perceptual features of behavior. An individual's behavior is not only phenomenally present in interaction but is also subject to view and appraisal by the other members of the group. If there are to be consequences involving these others, it is essential that there be a perceptual intake on their part. And so too must the individual perceive a group norm; the fact that it is manifestly there is not enough.

It is worth emphasizing that the focus here is upon how the individual fares in the group rather than upon more global consequences to the group. Two kinds of interlocking mechanisms are of concern: those giving rise to behavior in conformity with group demands, and those giving rise to status. The issues

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at stake may be put simply as follows: What produces conformity? And what allows for nonconformity?

SOME QUESTIONS ON CONFORMITY

Fundamental to these issues is the matter of determining *when* an individual may be said to be conforming. One may note that a twofold assumption underpins the usual view of conformity, i.e., that the individual is aware of the existence of a given group norm, and that his behavior in accordance with this norm is evidence of conformity. It is doubtful that both features of this assumption necessarily hold simultaneously. This being so, difficulties of interpretation will arise. If the individual were to be insensitive to the norm he could hardly be said to be conforming to it, whatever his behavior seemed to betray; correspondingly, a kind of "conformity" might prevail in terms of adherence to an incorrectly perceived norm; and thus, an evident failure to conform might or might not be "nonconformity" depending upon the accuracy of the individual's perception of the norm in the first place.

A related question concerns the individual's motivation. Is there a motive for nonconformity identifiable? Insofar as they are distinguishable, is it necessarily so, after all, that a conflict obtains between the individual's dispositions and the group's demands? Since behavior is taken to be more than a random event, the motivation for instances of conformity or nonconformity should be accountable, once the presence of an adequate recognition of the norm is established.

There remains too the question of who perceives a given behavior to be conforming, i.e., an external observer, a group member, or the actor himself. Employing a fixed-norm baseline for observation, as is often done, serves to obscure differential expectations which render conforming behavior for one individual nonconforming for another—with regard, that is, to others' perceptions *in situ*. Thus, the degree of familiarity with the unique properties of the group context is critical in verifying and understanding conformity.

NORMS, ROLES, AND GROUP EXPECTANCIES

The usual conception of conformity examined here requires some group referent and a standard of behavior abstracted therefrom and defined as a norm. Probably because many studies of groups have involved highly manifest behaviors, norms are conceived to be quite literally evident. On the other hand, in the related concept of role a recognition exists that the behavioral standard may not be manifest, but rather may be an *expectancy*.[3]

Though persisting, the distinction between norms and roles is neither essential nor easy to maintain (cf. **2, 16**). Roles are normative in that they involve some implicit shared expectancy among group members; and norms themselves, lacking visibility, may nonetheless dwell in expectancies. It is these expectancies, then, which may be normative, in the sense of typicality. Norms and roles are only distinguishable insofar as norms usually imply expectancies applicable to many persons, while roles are expectancies restrictive to one or a very few individuals in a group.

Objective observers might delimit common expectancies appropriate to group members in general from differential expectancies having reference to particular

[3] The term "expectancy" refers to another's perception of some object person (cf. **21**). What the object person then perceives to be the expectancy is quite important, but its locus is first of all in the "other." Reference is not made, therefore, to the term in Tolman's sense.

individuals as such. For the individual in the setting, however, manifest conformity probably comes about without regard to a separate awareness of norms as distinct from roles, but more likely in terms of behaviors which he perceives to be expected of him by relevant others, i.e., "doing the *right* thing."

In the world of daily interaction, the perception an individual holds of what relevant others expect of him is a singularly important determinant of his social behavior; and the degree to which an individual perceives the group to be rewarding serves to enhance or elaborate the effect produced by his motivation to belong. An alternative sequence may be seen to occur as well: motivation having reference to some fulfillment through the group serves to heighten the individual's perception of its expectancies.[4]

INDIVIDUAL VARIABLES

Granted that conformity derives from certain features of individual perception and motivation, it still remains necessary to identify these features more pointedly. In this formulation, there are four such to be noted: perceptual ability (P_a), representing a general alertness to the social stimulus field; perceptual error (P_e), with particular reference to group expectancies; motivation to gain or sustain social approval (M_a); and, motivation to take part in the focal activities of the group (M_g). Taken together, the latter two variables may be considered as the individual's motivation to belong to the group (M).

The perceptual variables can readily be related to personality typologies. Many of these, e.g., authoritarianism, rigidity, or empathy, appear to lend themselves to a reduction to perceptual function as a core element (cf. **1**, **3**, **19**). Terms like "perceptual rigidity," "perceptual defense," and "social imperceptiveness," often appear as concomitants of these broader characterizations; evidently, this element accounts for certain diversities in behavior which distinguish individuals from one another.

It is useful here, however, to recognize a differential between that which is given and that which is emergent, i.e., perceptual ability and perceptual error, though the interaction of the two is not challenged. The distinction basically is that the former serves as a parameter setting the lower limit on the latter. Thus, the minimum level of an individual's P_e is set by his basic capacity, P_a. This should not be taken as neglect of the potentials of learning, however. The concept of capacity introduced here may be understood to be similar to that of cognitive structures (cf. **15**). No assumptions are made about the source of the "capacity"; it is only significant as a feature of the individual which bears upon interaction. It seems reasonable to believe that some individuals have an initial advantage over others as regards accuracy in perceiving group expectancies.

Concerning motivation to belong, mention has already been made that it involves two continua: motivation specific to the activity—or instrumental features—of the group, M_g; and motivation rooted in a generalized need for social approval, M_a. This view cuts across a number of other motivational schema suggested elsewhere (cf. **4**, **8**, **9**, **12**, **23**), and is intended more as a resolution than a departure. Briefly, these other distinctions appear to involve an "activity focus" and an "other people focus." Activity involves others,

[4] The work on selective perception (e.g., **18**) sustains some such formulation, in general. But the linkage between motivation and perception has considerably greater complexity, as Bruner (**5**) has more recently pointed out.

of course, but not necessarily to gain their approval. What really seems to matter is the nature of the reward sought.

The approval variable might be viewed as a parameter of personality, but not one so static as to be unaffected by interaction, within certain limits. Since those members having interests which can only be satisfied through participation in group activity do not of necessity have a high need for social approval, and since those cast into groups of little positive activity valence to them may still require approval, it is possible that these variables may be related negatively or positively, depending upon the circumstances considered.

STATUS EMERGENCE

The foregoing points have concentrated on individual characteristics that absorb and deal with features of the social context. Ultimately, these have consequences in behavior, which in its turn has an impact upon the group. It is appropriate now to consider the implications of this process to the emergence of status.

At bottom, status may be taken to be an outcome of the group's differentiated perception of the individual, leading to a set of particularized expectancies regarding his behavior. This occurs as a function of certain of the behaviors or characteristics evidenced by the individual in interaction, which then yield a reconstruction of the group's perception of him. Cast in these terms, status has special value as a kind of middle ground in relating the individual to the group. It exists in the first place as a feature in someone's perceptual field, for without reference to a perceiver status has no intrinsic value or meaning in itself. And, similarly, role cannot be divorced from its perceptual locus; behavior is only appropriate to status insofar as someone perceives it to be so. Perceptual differentiation by the group has consequences, then, in terms of the behaviors it expects the individual to display.[5]

Though not necessarily the case, it is desirable to conceive of status within this framework as having hierarchical properties on some sort of group-acceptance continuum (cf. 7). This is by no means critical as a feature, but is of heuristic value. Still further, it is convenient to represent status as permitting greater latitude in the manifestation of behaviors which would be seen to be nonconformist for the other members of the group; we refer here to common expectancies, a term introduced earlier. The implications of this aspect of status are of especial relevance to what follows.

IDIOSYNCRASY CREDIT

Status will hereafter be considered to be an outcome of interaction referred to as "idiosyncrasy credit" (*C*). This represents an accumulation of positively-disposed impressions residing in the perceptions of relevant others; it is defined operationally in terms of the degree to which an individual may deviate from the common expectancies of the group. In this view, each individual within a group—disregarding size and function, for the moment—may be thought of as having a degree of group-awarded credits such as to permit idiosyncratic behavior in certain dimensions *before* group sanctions are applied. By definition, affiliation with the group—as perceived by the group—ceases when the individual's credit balance reaches zero.

[5] Implicit here is a concern with observable features of the individual. Thus, in the case of conformity, public manifestation is required; another position, taking account of both public and private conformity, has been advanced by Jahoda (Marie Jahoda. "Conformity and Independence: A Psychological Analysis." Unpublished, June, 1956).

It is noteworthy that this concept is applicable to the limited, artificially produced laboratory group as well as to the total society. And, since the individual may have simultaneous membership in many groups, he may be considered to have a distinct credit balance in all groups with which he is in some sense involved; in each case he has achieved some level of status. Affixed to this concept of "credit" is the further consideration that "debits" of varying magnitudes may be charged against the credit balance, depending upon the gravity and frequency of the idiosyncrasy manifested, and the credit level which the individual holds.[6]

Taking our society today as an illustration, one's credit balance very likely will be rapidly exhausted by publicly espousing Communist doctrine. In a different sphere, a fraternity man may experience comparable rejection by his peers for growing a beard, though other factors would come into play, so that for some individuals the consequences—in terms of group sanctions—would be disastrous and for others hardly disturbing. This requires some consideration of factors which determine the awarding of credit.

Among other determinants, the credit balance that a group member achieves depends upon the group, its function, and other properties to be considered below. It is useful for our purposes here to conceive of an "open system," i.e., an autonomous group providing focal activities, as well as free face-to-face interaction yielding expectancies; this would permit the simultaneous observation of an individual's behavior by all group members and the generation of impressions representing credit.

There are three general variables which can be delineated as determinants of these impressions. The first of these is alpha value (V_α), referring to the individual's task competence or performance in regard to focal group activities; the second is beta value (V_β), referring to characteristics of the individual not specific to these activities, e.g., status in a broader group, *bonhomie,* and the like; the third is immediate past idiosyncratic behavior (B), constituting a drain on credits.[7] It is not contended that credit is necessarily related linearly to these variables, nor is their very likely interrelationship ignored. They are doubtless intercorrelated, though of varying degrees of significance in generating or dissipating credits. As a generalization, value (V) tends to increase credit while idiosyncratic behavior (B) acts to decrease credit—though the potential for negative value exists, e.g., in the case of prejudice.

GROUP VARIABLES

From the foregoing it should be apparent that an individual can only be accurate in perceiving expectancies insofar as they are normative, in some modal sense, and are communicated. Two interrelated group variables which have importance in this regard are group attraction (A) and the communicality of any given expectancy (V).

The former variable may be thought

[6] Alterations upward or downward in credit may be conceived as a negative, monotonic function of credit balance. Thus, for the same idiosyncratic behavior or negative weight attached to value, the individual with high status loses less credit than the marginal individual of low status (cf. **20**).

[7] Still another variable related to credit balance, probably curvilinearly, would be the duration of the individual's affiliation with the group over time. This has been disregarded, since it is useful to deal with individuals as though they have been in the group for an equal period of time, more particularly from its inception. It is also likely that the degree to which the individual is "visible" may alter the effects produced by his value and idiosyncratic behavior.

of as "cohesiveness," a term more usually applied. But since this term may have at least several operational meanings, it is preferable to specify two kinds of literal attraction, or an aggregation of these: attraction to group members, and attraction to focal group activities. This duality follows the M_a and M_g distinction made earlier in connection with motivation to belong. Although one may deal with a nondiscriminate aggregation of these, it is quite true that the sum of individual attractions in the group may be based predominately in M_a needs or M_g needs, and that differences in the emergent characteristics of groups thus constituted will be evident. Thus, where group attraction derives mainly from M_a one might predict it would be more stable than where its source is mainly M_g, since the latter variable is more temporally based.

Communicality is conceived to be directly related to group attraction in a mutual dependency; it refers to the degree to which an expectancy is literally communicated, i.e., made evident, and bears a relationship to both relevance and communication variables, as they have been introduced in other formulations (cf. 6, 22), though this is by no means a complete statement. Studies of leaders' ability to estimate group attitudes, for example, have yielded highly conflicting results (cf. 21, p. 268). Where attitudes are "relevant," leaders *may or may not* be superior to nonleaders in their respective estimates; where leaders *are* found to be superior to nonleaders in estimating relevant attitudes, this has been ascribed to the heightened social sensitivity of leaders or alternatively to the proposition that leaders are instrumental in the shaping of group attitudes and hence tend to know them better.

Whatever the explanation chosen, there is reason to contend that the variable of relevance may not be the most fruitful one for purposes of study. If one were disposed to test the tenability of the hypothesis that leaders have this greater social sensitivity, it would seem desirable not only to raise the question of whether leaders, and others, tend to use their own personal attitudes as an anchorage for estimating group attitudes, but to ask in addition whether this exists independently of the degree to which given attitudes actually are foci of communication within real groups. Another approach, accordingly, might be to utilize some index of the degree to which a specific attitude —or an expectancy—actually evidences itself in a given group. There is utility, then, in introducing the operationalizable property "communicality."

Though a level of communicality may characterize a group, the particular center of interest here resides in a given expectancy. One may venture in this vein that the communicality of an expectancy will be at a lower relative level than that of other expectancies, if it is less applicable to the group as a whole; common expectancies ought to have higher communicality than the differential expectancies associated with increased status. Since communicality rests on behavior, it may be seen to follow, too, that interpersonal interaction results in higher communicality. Through a related mechanism, interpersonal interaction may contribute to group attraction (cf. 11, 17).

SUMMARY OF VARIABLES

For convenience, the variables described may now be set forth definitionally.

B—Idiosyncratic behavior, i.e., any group member's behavior which may be perceived by the group to deviate from a given group expectancy.

C—Idiosyncrasy credit, i.e., the extent to which a given group member's idiosyncratic behavior (*B*) is allowable, in terms

of gravity and frequency, before group sanctions are applied.

V_α—*Alpha value,* i.e., the weight assigned the current performance of a given individual, which may be perceived by the group as bearing upon its focal activities, e.g., task competence.

V_β—*Beta value,* i.e., the weight assigned the characteristics of a given individual which may be perceived by the group, but are not specific to its current focal activities, e.g., status external to the group.

P_a—The *perceptual ability* of a given individual, in the sense of a capacity to perceive events and relationships in the social field.

P_e—The *perceptual error* of a given individual in perceiving events or relationships in a particular social field, e.g., group expectancies.

M_a—The *motivation* of a given individual to affiliate with a given group, in terms of a gaining or sustaining *social approval.*

M_g—The *motivation* of a given individual to affiliate with a group, in terms of interest in focal *group activity.*

M—Individual motivation to gain or sustain membership, i.e., some composite of a given individual's motivation of both the M_a and M_g variety.

A—In general, *attraction of the group to its members,* i.e., some aggregate of all group members' *M*.

Y—The *communicality of a given group expectancy,* in terms of the degree to which a given expectancy is evident.

SCHEMATIC REPRESENTATION

In Fig. 1 our symbolic notation has been employed to represent relationships schematically. Since a sequential pattern is of particular importance, a time dimension is involved throughout; thus, subscripts are introduced to indicate the time interval to which reference is made; e.g., t_1 is read as the first time interval; or, P_{e_1} as perceptual error in the second time interval.

The system originates at the top with group attraction as a motivational context, and three individual variables, perceptual ability, motivation to gain or sustain in social approval, and motivation [illegible]ference to the group's activity. [illegible] next level, group attraction has [illegible]ise to the communicality of cer[illegible]n expectancies which are then perceived by the individual, thus yielding a perceptual error; and motivation to belong has been aggregated at this level, as well. Beta value is also introduced to signify the group's perception of the individual's characteristics, e.g., pleasant appearance.[8]

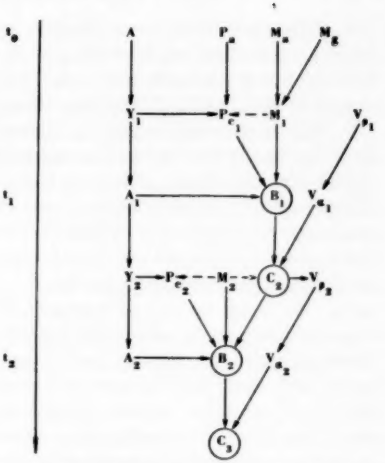

FIG. 1. Schematic representation of mechanisms demonstrating relationships over time.

Moving down in time, the individual's idiosyncratic behavior during the period just elapsed has been generated by his error in perceiving expectancies together with his motivation—within the constraints imposed by the level of group attraction. The group's percep-

[8] The term "group perception" refers to an abstraction; it is unlikely that all members of a group will perceive a given feature of an individual identically; the intent therefore is only to suggest a modal tendency. However, some very recent research has illuminated just this point and given credence to the general conception of differentially-determined value (cf. **14**).

tion of the individual's contribution to its focal activities, alpha value, is influenced by the immediately prior perception of his characteristics, beta value.

In the next stage, status is generated—in the form of group-awarded credits—by the effects of behavior relative to expectancies and the sequence of beta value to alpha value to credits. At this point the full set of interactions are in play, with credits affecting beta value; the latter serving as a repository of group perceptions of the individual's characteristics; perceptual error and motivation are reintroduced for this new phase, with the former affected by communicality of expectancies. Idiosyncratic behavior is subsequently determined by available credits, as this is checked by motivation, in particular, and perceptual error.

Discussion and Implications

Early in interaction, conformity to group expectancies serves to maintain or increase status, particularly as it is seen to be combined with manifest contributions to the group; at a later phase, however, the status thus generated permits greater latitude for idiosyncratic behavior. Thus, if an individual conforms to expectancies early in his exposure to the group and if he shows characteristics of competence, he accrues credits. For evident deviations from expectancies, or poor performance, he loses credits. If he exhausts his credit balance completely, pressures are applied to remove him from the group, or, at the very least, he is no longer perceived to be a member. At the other pole, if he continues to amass credits he attains a threshold permitting deviations from common expectancies, but with constraints imposed by newly differentiated expectancies.

The apparent paradox—that leaders both conform to group norms and yet may act to alter them by an exercise of influence—may be explained by reference to this sequential process. In this regard, it should not be supposed that an abundance of credits must lead perforce to influence. While an individual thus endowed has the potential to display more idiosyncratic behavior than others, he might not do so, nor would he of necessity become a leader thereby. Some further points of clarification are in order.

It is easy enough for the individual to continue to do habitually that which is rewarded by relevant others, so long as expectancies remain relatively stable. Consider the state of affairs which holds, however, in the case of the person who has marked status mobility in the group. He cannot simply continue to redisplay behaviors which were appropriate to the group's earlier expectancies, because the expectancies applicable to him are now altered in keeping with his rising status. Other things being equal, this suggests two features appropriate to the attainment of status in an open system: (*a*) accuracy of social perception; and (*b*) modifiability of behavior. Insofar as the incipient status person is attuned to the altering group expectancies and is capable of reacting appropriately to them, his status will very likely move upward. The relationship of these points to research on leadership is noteworthy; for example, the proposition that leaders have a heightened sensitivity to certain properties of the social context is in consonance with the foregoing. Note, too, that whether or not the leader has actually created a feature of the context with which he is then familiar, he may well have evidenced perceptual accuracy in an earlier phase, as he rose to leadership. Accordingly, the finding of Talland (22) that leaders are only better in judging norms where they have had a part in their evolution is not inconsistent with a finding like that of Chowdhry and Newcomb (6) to the ef-

fect that leaders have superior social perception; it would appear that the time phase under study is critical in yielding one process or the other.

Previously the point was made that the high-status person could effect changes in the common expectancies of the group because he has latitude for the manifestation of what would be seen to be nonconforming behavior for others. But, in contrast, the expectancies regarding the role itself are less amenable to alteration by the incumbent. Hence, the leader could readily lose credits and find his influence diminished if he were to show idiosyncratic behavior in terms of expectancies associated with his role. Regarding such deviation, we may conceive of one requirement which is quite likely significant, i.e., perception by the members of the group that the leader's motivation to belong be both high and sincere. Should this condition not be fulfilled, status may be threatened. To take another illustration, innovation by the leader may be of high valence to the group. It is conceivable that this could yield the seeming anomaly of a leader who, in the face of this expectancy, adopts a passive and ostensibly safe course, but loses status.

Leadership status, therefore, assuredly demands conformity to the group's expectancies regarding the role, but still leaves the leader with sway in the sphere of common expectancies associated with members at large. The leader may deviate from these, or bring about their reconstruction, if his prior activities have generated an appropriately high level of credit.

The motivational aspects of this process require consideration in terms of the individual set against the background of the group's activity. The restraining effect of M on the expenditure of C has been accounted for in the foregoing. What is more to the point, however, is the fact that a person with M_g will more likely achieve status in a largely M_g group than will a person with M_a; the status achieved, therefore, is a part function of the congruence of the individual's motivation with the generalized character of the motivation extant in the group. Since motivation is related, as well, to performance and other characteristics represented by alpha value and beta value, it is to be regarded as a key element.

If the group has a primary focal activity, then presumably M_g becomes more highly valued, particularly in combination with alpha value. At the other extreme, i.e., where the group activities are quite diffuse, M_a becomes important in combination with beta value. We may conceptualize groups of the M_a variety as being essentially "socially minded." With a minimum group-centered function requiring broad participation, the person having beta value is more likely to achieve status through being well liked; in the M_g group, to the contrary, alpha value becomes critical and task competence has greater weight in determining status. The relationship of this to the current situational view of leadership is evident.[9]

Whether group members do distinguish between value of one kind or another can be inferred from the literature of sociometry. Evidence will be found there indicating that individuals can give scaled evaluations of their peers with quite adequate discriminations between those they like, those they consider competent, and so forth. Though these sociometrically based status continua are likely to be related, as we have previously noted, they are by no means in universal, one-to-one correspondence (cf. 10). Viewing status in the aggre-

[9] For example, the finding of Jennings (13) that leadership and popularity are highly related in her groups can be considered in this framework.

gative, credit-amassing sense still allows for the integrity of the roots which feed it. Recent experimental evidence on the basis for shifts in the group's perception of a member also accords with this conception (cf. **14**).

Certain of the assumptions made here —e.g., that an individual will have a level of credit reposing unitarily in others' perceptions of him, and that he may know and make use of the credits at his disposal—are only approximations of reality. Their literal tenability is not crucial to the mechanisms postulated, however. One could argue that the individual operates *as if* these assumptions were in fact true: the "they" commonly invoked to denote the upholders of some social pattern are never quite as homogeneous as the term suggests; but, to the individual, the use of "they" to represent a supposed uniformity is a necessary convenience as a basis for behavior. Furthermore, in accordance with this position, the individual apparently does react differentially to what he believes to be the view of him held by the "they," as Dittes and Kelley (**7**) have demonstrated by manipulating the level of group "acceptance" which an individual is permitted to sense. In general, then, it appears that the individual seeks to know where he stands and does the best he can with the information available to him. These conceptions therefore do no violence to the reality with which the individual deals, but rather describe this reality in terms congruent with his concern.

SUMMARY

Beginning with the consideration that social behavior depends upon attributes of the individual, conditions of the situation, and inputs to a dynamic system arising from their interaction, a theoretical conception relating conformity and status is presented.

The major mediating construct introduced is "idiosyncrasy credit," taken to be an index of status, in the operational sense of permitting deviations from common "expectancies" of the group.

Credits are postulated to increase or decrease as a function of the group's perception of the individual's task performance and generalized characteristics, and of his "idiosyncratic behavior," i.e., deviations from its expectancies. Though increases in credit are seen to permit greater latitude for idiosyncratic behavior, motivational and perceptual states of the individual, and group-level phenomena, are also considered.

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ERRATUM

In the article "Uncertainty and Conflict: A Point of Contact Between Information-Theory and Behavior-Theory Concepts" by D. E. Berlyne in the November 1957 issue of this Journal (*Psychol. Rev.,* 1957, **64**, 329–339), there are two errors. On p. 332, in Property 5, "$= E_1$" should be substituted for "> 0," and on p. 333, Expression 2 should read "$\Sigma_i E_i(\log(\Sigma_i E_i) - \log E_i)$."

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